

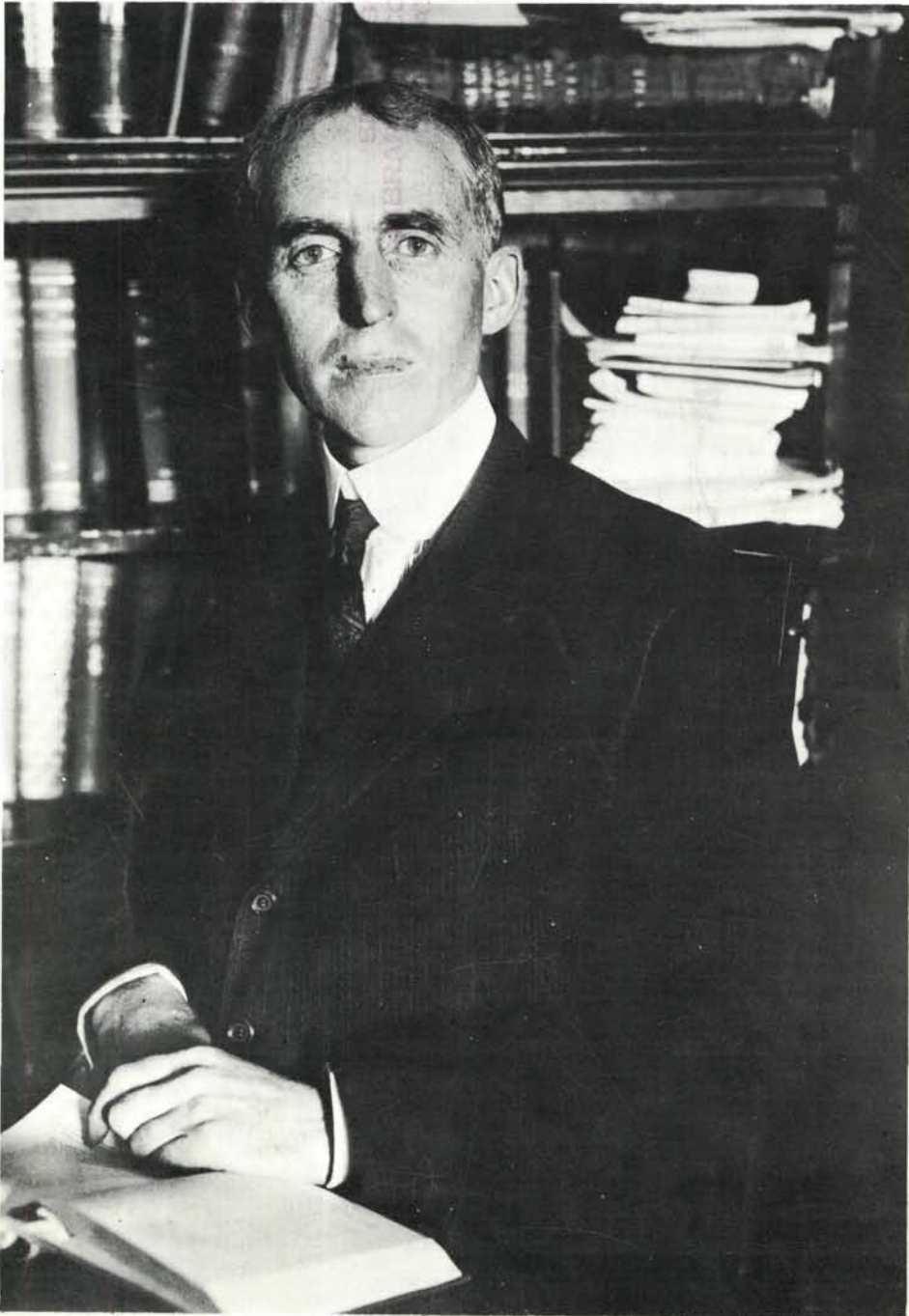
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## Power and Authority in American History: The Case of Charles A. Beard and His Critics

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JOHN PATRICK DIGGINS

Why try to think? Perhaps Henry Adams was right: "Silence is best." What to think? That was my problem twenty-three years ago, and it remains unresolved. Yet for some strange reason I do not keep the silence.

—Charles A. Beard, 1936

Man doesn't really exist until he is fighting against his own limits.

—Ignazio Silone, 1937<sup>1</sup>

PERHAPS NO HISTORIAN HAS BEEN SO ESTEEMED in one period and so systematically criticized in another as Charles A. Beard (1874–1948). Praised in the Progressive era as the prophet of the "New History," he was dismissed in the 1950s as a well-intentioned but rather simple-minded historian who had led astray an entire generation of scholars. Beard went wrong, a chorus of consensus historians now declared, in insisting that the American Constitution should be explained historically as a product of social conflict emerging from the forces of economic determinism. By the 1960s, Beard's reputation stood "like an imposing ruin on the landscape of American historiography."<sup>2</sup> After that pronouncement by Richard Hofstadter, nothing more, it seemed, needed to be said about the most controversial American historian of the twentieth century. This essay, however, challenges that verdict. It raises the possibility that much of Beard's work, properly understood, is less a historiographical ruin than an enduring monument to his involvement with the problem of power and authority in American history. By focusing both on Beard's critics and on his own ultimate concerns, I endeavor to show that some of the criticisms and categories associated with his reputation warrant re-examination and his own concerns warrant renewed respect.

In the years between the two world wars, Beard's reputation was so firmly established that the adjective "Beardian" was not only considered a compliment

I would like to express my appreciation to the following persons who offered helpful criticisms of an earlier version of this essay: Joyce Appleby, Joseph Ellis, Robert Huberty, Gerald Meaker, and Sheldon Wolin.

<sup>1</sup> Beard, "Little Alice Looks at the Constitution," in his *Jefferson, Corporations, and the Constitution* (Washington, 1936), 92; and Silone, *Bread and Wine* (1937; reprint edn., New York, 1963), 179.

<sup>2</sup> Richard Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968), 344.

but denoted a respected school of thought as well. The term stood for a liberating methodological realism that inspired historians to ask disturbing questions about the past and thereby encouraged scholars to write history not as it had been taught but as they discovered it to be. Although he had several predecessors, Beard dramatically broke the spell of conventional political history with *An Economic Interpretation of the Constitution* (1913). After investigating the property holdings of the delegates to the Constitutional Convention in Philadelphia, Beard proclaimed that the proponents of the Constitution had maneuvered undemocratically to foist upon the nation a new federal system of government that represented the interests of the minority mercantile classes rather than those of the great mass of small farmers and debtors. American history, it now seemed, could be told as it “really” was.

Beard's thesis came as a revelation to scholars and intellectuals who hailed it for undermining the patriotic banalities of nineteenth-century historiography. Conventional wisdom decreed that the Constitution was the work of Providence's guiding hand, the fruit of the genius of Teutonic legal traditions, or the final compromise of the contending theories of states rights and national sovereignty. Such traditional explanations now seemed too formal and abstract, too divorced from the reality of everyday political experience, to offer an adequate account of human behavior even in the eighteenth century. Indeed, not only was the generation of the 1930s happy to see the “superstructure” of religion and law subjected to Beard's “scientific criticism,” it was even happier to be told that the Founding Fathers had themselves subordinated abstract, doctrinal issues to the reality of political power and economic interests in an attempt to control class struggle. Max Lerner, examining earlier reviews of Beard's book, wondered how historians could refute an author who drew his citations from the Founders themselves. “What could you do with a man like that?” asked Lerner in 1938. “You couldn't ignore him. You could only rage against his indecency, question his patriotism, accuse him of fomenting class hatred—and bide your time.”<sup>3</sup>

Time finally ripened in the 1950s, when Beardianism became almost synonymous with Marxism and both schools of thought were rejected by scholars in search of a fresh interpretive scheme to replace the worn-out concept of class conflict. The critics who came to the forefront of American historiography in the 1950s and early 1960s settled upon the notion of ideological consensus and produced an impressive body of work that makes some of Beard's categories seem, to paraphrase Jeremy Bentham, like simplicity on stilts. Scholars of early American history have shown that Beard exaggerated the intensity of the conflict over the Constitution, misconstrued the motives for its adoption, was naively preoccupied with the undemocratic procedures by which it was conceived and ratified in a period when mass democratic participation was unknown, and severely underestimated the real concern with political unity, economic development, and diplomatic security that led the framers to replace the Articles of Con-

<sup>3</sup> Lerner, “Beard's ‘Economic Interpretation of the Constitution,’” in Malcolm Cowley and Bernard Smith, eds., *Books That Changed Our Minds* (New York, 1938), 153–54.

federation with a new constitution. Beard also failed to comprehend that the Constitution aimed to preserve liberty, not merely to protect property, and he wrongly depicted the goals of the framers as different from those of the people, an interpretation that convinced a generation of American historians that the framers consciously intended to foster the development of economic power that a century later brought forth the triumph of corporate capitalism.

Beard's critics have, in large measure, been correct and thereby have greatly advanced our understanding of the eighteenth-century mentality. But this essay is not concerned with refuting criticisms of Beard's schematic categories of the social groups supporting and opposing the Constitution. Neither does it attempt to resurrect the Beardian point of view in its entirety, nor to insist upon the use of "conflict" as opposed to "consensus," at least in the relations among American social classes. My concern is not with conflicts that may have existed among classes or factions but rather with tensions that exist among different ideas or within the same body of ideas. Although conflict may not have characterized existing class relations at the time of the Constitution, it did lie embedded in eighteenth-century ideas, and Beard's mistake, the temptation that awaits any historian who cannot resist the appeal of Marxism, was to translate conflicts that inhered in political principles into conflicts that supposedly divided social classes.<sup>4</sup> By focusing on ideas, I hope to swim against the current of anti-Beardian criticism without getting caught in a riptide of minutiae regarding the empirical data of social history. For there remain several basic criticisms of Beard that have to do with ideas as attitudes and values that supposedly explain human conduct. These criticisms, which emerged in much of the "neo-Whig" scholarship of the 1960s,<sup>5</sup> may be broken down into five indictments:

1. Beard's *Economic Interpretation of the Constitution* amounts to little more than a "Progressive tract,"<sup>6</sup> an embarrassing anachronism that reads into the eighteenth century a conflict between the ideas of property and liberty that has no basis in the Constitution.
2. Thus, in depicting the Revolutionary ideals of 1776 as yielding to the conservative counterforces of 1787, Beard erred when he interpreted the Constitution as a violation of the spirit of the Declaration of Independence.
3. Beard failed to consider contemporary Whig ideas about eco-

<sup>4</sup> Some historians have treated Beard as a closet Marxist. Douglass G. Adair suggested that Beard chose the strategy of citing Madison instead of Marx as a "device quite self-consciously adopted of wrapping himself in the American flag"; Adair, "The Tenth Federalist Revisited," *William and Mary Quarterly* [hereafter, *WMQ*], 8 (1951): 48-67. The more important point, however, is that Beard profoundly misinterpreted Madison as a theorist trying to come to terms with class conflict, and Beard's critics have tended to compound the error by trying to deny the existence of conflict. As Madison recognized, conflict does not contradict consensus but actually reinforces it by demonstrating that men are alike in their desires and contend with one another because they are pursuing the same ends regardless of the means. Indeed, the framers formulated a theory of conflict that determined the course of American history in terms of consensus. For their notion of conflict cannot be understood apart from the "interests" that logically presuppose the wants that people have in common. If there were no consensus about the value of property, there could be no conflict about it.

<sup>5</sup> Jack P. Greene used the phrase "neo-Whig" in his valuable anthology, *The Reinterpretation of the American Revolution, 1763-1789* (New York, 1968).

<sup>6</sup> Greene, *The Reinterpretation of the American Revolution*, 59.

nomic "independence," political "virtue," and social "deference" that made the rule of property in the colonial era as acceptable as it was.

4. Beard's neglect of eighteenth-century intellectual history guaranteed his failure to comprehend that the Founders were acting upon the basis of "ideas" rather than "interests."
5. Beard erroneously interpreted James Madison's theory of government as an expression of "economic determinism" in order to validate his own theory of history.

I shall attempt to answer these criticisms by setting forth five opposing theses:

1. The conflict between property and liberty was implicit in the Constitution and *The Federalist*, due to the changing meaning of the two concepts themselves as the focus of political thought after the Revolution shifted from figures like Thomas Paine and Thomas Jefferson to theorists like Madison and Alexander Hamilton.
2. Therefore, the Constitution does indeed depart in significant ways from the more liberating and morally grounded principles of the Declaration.
3. The arguments of the neo-Whig historians regarding "independence," "virtue," and "deference" were not part of the thinking that went into the making of the Constitution and thus today have little relevance to Beard's thesis.
4. The framer's use of political ideas does not preclude the possibility that they were ultimately concerned with economic interests, which they regarded as the mainspring of human action and hence the most basic and reliable ingredient of political obedience.
5. Far from being an "economic determinist," Beard was an anguished moralist who wanted to see man free and responsive to ideals higher than mere interests and power, ideals the framers themselves denigrated when writing the Constitution.

In discussing these five theses, my ultimate aim is not so much to answer Beard's critics as to enable us to understand one of Beard's own ultimate concerns—authority. Throughout his intellectual life, Beard was preoccupied with problem of authority, with legitimate, rightful government that ought to be based on political and moral "ideas" rather than on economic and class "interests."<sup>7</sup> An

<sup>7</sup> "It is impossible to survey the history of this long conflict over the nature of the Constitution and States' Rights without coming to the conclusion that, in the main, theories followed interests instead of controlling them"; Beard, *Jefferson, Corporations, and the Constitution*, 73-74. The relationship between ideas and interests could well be the central theme of Beard's thought, a perplexing theme he could never resolve; nor can his critics. Convinced that ideas lagged behind interests, he also seems to have been certain that interests could only be mediated through ideas: "As Kurt Riezler says, there are no interests without ideas, and no ideas without interests"; Beard, *The Nature of the Social Sciences* (New York, 1934), 60. Beard was fond of declaring that "the movement of ideas and interests, called history, proceeds"; *Jefferson, the Corporations, and the Constitution*, 81. But he never precisely defined his use of the elusive terms "ideas" and "interests" even though they constituted his definition of "history." In his works on the Constitution and the Supreme Court, he understood that ideas about authority could be discredited by disclosing the interests behind them. Yet, once authority had been



*Economic Interpretation of the Constitution* attempts, among other things, to explain the alienation of economic power from political authority. In that great classic, Beard was concerned with legitimacy, with the historical origins of institutions, and, specifically, with the forces and motives that brought the Constitution into being. In searching for the origins of the Constitution's authority in the material forces that led to its creation, such issues as property and liberty and ideas and interests were crucial to Beard, and they are by no means irrelevant to us. If property and liberty were almost identical in meaning because one presupposed the other, as Beard's critics have argued, then explaining how one could become alienated from the other would be difficult within the context of an ideology that valued rights as unalienable. How could property as an expression of liberty also come to contradict liberty by constituting a source of economic power over the people, whom the framers of the Constitution purported to represent (and nineteenth-century America generally believed they did), the ultimate source of authority?

Another matter crucial to Beard's project was the distinction between ideas and interests. If ideas and interests were identical because each required the other for its realization (the assumption of some aspects of eighteenth-century thought), then clearly ideas could not be invoked to subject the power of economic interests to the constraints of political authority. At the most, ideas could express, not control, interests. Only interests could control interests through the strategy of mutual counterpoise of contending factions. And, insofar as interests operated through the ideas that expressed them, ideas could not constitute a judgment on the activity of interests by virtue of appealing to a norm independent of the activity judged. Although *The Federalist's* authors were fully aware of the inefficacy of ideas as a means of judging and controlling the behavior of factions, Beard's critics have dwelled upon the framers' political ideas in an attempt to discredit the "economic interpretation" of the Constitution. Beard, however, understood perfectly that the framers looked not to political or moral ideas to preserve the republic but to the "machinery of government," to a system of checks and balances in which there could be no appeal to ideas apart from interests.

But Beard also understood—and here is the issue that separates him from both the framers and his critics—that the older Lockean notion of sovereign political authority expressed directly in the will of the people, a concept that had

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divorced from morality, Beard still had to face the inescapable reality of interests and power without any normatively binding idea, principle, or theory that could subject power and interests to knowledge and control. Beard was too much a relativist and naturalist to believe that a new source of legitimacy might be found in a moral principle, a religious precept, or a philosophical tenet, yet he realized that interests could not be controlled without some ultimate legitimating rationale that stood outside the movement of interests and, thereby, constituted a judgment of it. He looked to "ideas" to fulfill this need for moral authority; he also looked to ideas to liberate humankind from the reality of interests, from those deterministic forces that exist beyond man's will and, therefore, control his behavior. "If history is nothing except a 'chain of causes' and individuals are merely atoms in the flow of things, then all of us, students and teachers alike, are mere puppets in a mechanical play. If, on the other hand, history-as-actuality is made in part at least by thought and purpose—by ideas—then there is room in the world for will, design, courage, and action, for the thinker who is also a doer." *The Nature of the Social Sciences*, 61. Beard opposed "ideas" to "interests" to find "room" for freedom and authority.

been implied in the Declaration of Independence, became obscured in the Constitution, fragmented and deflected to the point where consolidated economic interests could later operate free of popular control that might have been exercised through the political state. This prospect of a crisis of political authority when it confronted alienated economic forces may have eluded the framers. But subsequently Beard's contemporary Henry Adams and later our own contemporary Hannah Arendt recognized the dilemma: a political state without a defined center of authority contending with a centralizing economy, a state that had "abolished" the concept of sovereign authority and thus had become politically powerless to control economic power.<sup>8</sup> Beard's effort to demonstrate the autonomy of judicial power and the hegemony of economic interests in American history was an effort to demonstrate how power and interests had escaped the authority of institutional controls spelled out in *The Federalist*. If Beard's critics have been right to point out that the framers did not intend to allow any power, whatever its form, to elude the delicately balanced mechanisms of government, Beard was equally right to point out that there was nothing in the theories of the framers—neither their ideas about liberty and property nor their uncertain thoughts on the ultimate locus of authority—to guarantee that such a state of affairs cannot come to pass.

This essay, then, is about two different, yet closely related, topics: Beard's immediate critics and Beard's ultimate concerns. Many of his critics have implied that, had Beard steeped himself in the eighteenth century, his concerns would have disappeared because the conflicts he sensed in his own era did not exist in the era of the Founders.<sup>9</sup> But Beard did not err; the conflict between liberty and property that led to the alienation of power from authority had its theoretical origins in the eighteenth century, and Beard correctly focused on the Constitution and *The Federalist* to illuminate the nature of a problem he could disclose but not resolve. A clarification of the theoretical problems in the five criticisms made against Beard may help delineate the achievement of his great classic and demonstrate why his quarrel with American history should once again deserve our appreciation.

THE DENIAL OF A CONFLICT between liberty and property—the first criticism—generally contains two claims. Insofar as the American colonists believed in both principles and, indeed, fought a revolution both to defend property and to preserve their liberties as a natural right, no conflict could have existed between such eighteenth-century principles. And, critics have contended, had Beard

<sup>8</sup> Adams, *The Great Secession Winter of 1860–61 and Other Essays*, ed. George Hochfield (New York, 1958), 189, 193–94; and Arendt, *On Revolution* (New York, 1963), 139–218.

<sup>9</sup> The claim that Beard read back into history the concerns of his Progressive era presumes that he himself was a Progressive. Yet, for reasons I cannot explore here, it seems to me that Beard had little in common with notable Progressive intellectuals like John Dewey, Oliver Wendell Holmes, Jr., and Thorstein Veblen. Indeed, Beard's greatest admiration went out to a writer who was neither a Progressive nor a pragmatist, who, in fact, scorned the claims of intelligence and questioned the idea of progress—Henry Adams. The notion of Beard as a Progressive is another example in intellectual history of the tyranny of paradigms.

been aware of the meanings of the words themselves in their precise historical context, he would have avoided such anachronistic dualisms that handicapped the Progressive mind.<sup>10</sup> The conflict, admittedly, may not have been as clear-cut as Beard presented it, especially in his use of such unfortunate categories as "agrarianism versus capitalism" to depict a popular citizenry—supposedly representing liberty—arrayed against a commercial oligarchy—purportedly standing for property.<sup>11</sup> But conflict cannot be easily dismissed by absorbing it into consensus simply because we have discovered what Beard should have realized all along—namely, that different groups and factions shared similar interests and values. Conflict inhered not so much in irreconcilable class tensions as in the changing and transforming nature of ideas themselves. Thus, even if Beard was guilty of not studying the contemporary meaning of terms, neither he nor his followers were compelled to revise their position. The meaning of such terms as "liberty" and "property"—to say nothing of "sovereignty," "authority," "representation," "rights," "power," "consent," "natural law," and "the people"—was certainly not fixed and clear in the eighteenth century. Between the Revolution and the Constitution the meaning and usage of familiar political ideas changed in unfamiliar and often incompatible ways.<sup>12</sup>

The Revolutionary generation tended to adopt the familiar Whig and Dissenter ideas of English thinkers, who had broadly defined property as the combination of lives, liberties, and estates, thereby embracing the concept of liberty within property.<sup>13</sup> *The Federalist's* authors, however, confined property to economic pursuits and liberty to the political realm in order to prevent popular majorities from using political means to achieve economic ends. Property expressed, among other things, a person's drive to possess that with which he is not necessarily endowed; liberty, in contrast, was an inherent right that enabled citizens to protect themselves from the activity of others that may infringe upon their rights and possessions. This tension was recognized in Madison's distinction between "persons" and "property," both of which had to have their rights represented in government. "The personal right to acquire property, which is a natural right, gives to property, when acquired, a right to protection, which is a social right."<sup>14</sup> The conflict between property as a means to life and liberty as a means not only to freedom but to protection represents a tension between two values that was never completely reconciled, a tension that deserves to be dramatized if only because Beard did not fully grasp it and his critics have

<sup>10</sup> Edmund S. Morgan, *The Birth of the Republic, 1763–89* (Chicago, 1956), esp. 129–57. Also see Edmund S. Morgan, "American Revolution: Revisions in Need of Revising," *WMQ*, 14 (1957): 3–15; Hofstadter, *The Progressive Historians*, 223–43; and Adair, "The Tenth Federalist Revisited," 48–67.

<sup>11</sup> Beard, *An Economic Interpretation of the Constitution* (New York, 1935), 1–51, 73–151 *passim*.

<sup>12</sup> For example, for two excellent analyses of the changing meanings of "sovereignty," see Bernard Bailyn, *The Ideological Origins of the American Revolution* (Cambridge, Mass., 1967), 160–229; and Gordon S. Wood, *The Creation of the American Republic, 1776–1787* (Chapel Hill, 1969), 344–429.

<sup>13</sup> Bailyn, *Ideological Origins of the American Revolution*, 22–93; and Caroline Robbins, *The Eighteenth-Century Commonwealthman: Studies in the Transmission, Development, and Circumstances of English Liberal Thought from the Restoration of Charles II until the War with the Thirteen Colonies* (Cambridge, Mass., 1959), 3–133, 378–86.

<sup>14</sup> Madison, *The Writings of James Madison, Comprising His Public Papers and His Private Correspondence, including Numerous Letters and Documents Now for the First Time Printed*, ed. Gaillard Hunt, 9 (New York, 1910): 360–63.

too readily dismissed it. This tension between social and natural rights—between one's right to use government to exclude others from using their collective power to acquire wealth politically and the right of others not to be excluded, between, in short, the possession of property and its pursuit—became more pronounced as the thought of David Hume replaced that of John Locke in *The Federalist*. Now the desire for property was referred to as a "passion" that compels man to seek something beyond himself, not as a "liberty" inherent within the nature of man and thus an unalienable right.

The issue of a conflict between property and liberty does not necessarily turn on the existence of clear-cut divisions between the supporters and opponents of the Constitution. Beardian critics Jack P. Greene and Cecelia M. Kenyon have rightly described the Anti-Federalists as distrustful of liberty and the unrestrained power of the people, while Jackson Turner Main seems to have resorted to Progressive categories in depicting the Federalists as representing the forces of aristocracy and the Anti-Federalists the impulses of democracy.<sup>15</sup> In *Economic Origins of Jeffersonian Democracy* (1915), Beard also showed, as did his later critics, that the Anti-Federalists challenged neither the Federalists' commitment to property nor their constitutional restraints on democracy.<sup>16</sup> Thus, in this instance at least, Beard was in the ironic position of agreeing with his critics that both eighteenth-century factions could—and did—hold roughly similar views on property and liberty. Yet the conflict between the two principles remains, and it does so because it lies embedded in the idea of property itself.

Beard presumably glimpsed the problem when he noted that the Constitution not only refrains from defining the nature of property but also denies itself the authority to do so.<sup>17</sup> At issue is whether property should be regarded as a natural or as a social or civil right, a right to the fruits of one's labor and access to nature's bounty or a right to call upon government to protect one's possessions. In *The Federalist*, property generally requires protection by government as an "interest" because its possession derives from the diverse and unequal "faculties" of acquisition and not necessarily from productive labor that would legitimate property as a natural right. In this respect property and liberty remained potentially in conflict not because they were incompatible as original principles but rather, as the shift from Locke to Hume suggests, because their meanings underwent significant changes from the period of the Revolution to that of the Constitution. Specifically, what had changed was both an older moral philosophy of property and an older majoritarian philosophy of liberty, the Lockean connection that had been lost to the Scottish turn in American political thought.

According to Locke, man had the natural right to property because through labor man preserves life. In *The Federalist*, however, Hamilton and Madison did

<sup>15</sup> Greene, *The Reinterpretation of the American Revolution*, 71–72; Kenyon, "Men of Little Faith: The Anti-Federalists on the Nature of Representative Government," *WMQ*, 12 (1955): 3–43; and Main, *The Antifederalists: Critics of the Constitution* (Chapel Hill, 1961), esp. viii–xii, 1–20, 249–81.

<sup>16</sup> Beard, *Economic Origins of Jeffersonian Democracy* (1915; Free Press edn., New York, 1900), 415–67.

<sup>17</sup> Beard, *An Economic Interpretation of the Constitution*, 176.

not rely on Locke's theory of ownership, where man acquires property by the labor of his hands as he wrests things out of their natural condition and thereby brings value into the world by appropriating nature.<sup>18</sup> Instead, Madison rested property not on the specific value of labor but on the vague "faculties" of acquiring it, which to a Humean could mean anything from skill to stealth. From government's protection of these "different and unequal" faculties results "different degrees and kinds of property" and "a division of society into different interests and parties."<sup>19</sup> Thus, in *The Federalist*, property is treated as an "interest" that requires protection by government, not primarily as a natural right the legitimacy of which lies in its historical origins, in the free act of labor that creates value and thereby promotes life. As simply a "present possession," to use Hume's language, the idea of property loses its moral connotation, and perhaps this lost Lockean dimension makes the Constitution stronger on the right of property than the right to property.<sup>20</sup>

The idea of liberty also underwent a significant change by the time *The Federalist* came to be written. Although Hamilton and Madison bowed to the familiar notion that people are "the fountain" of authority, the structure of the new federal system worked against that older Lockean principle of liberty. For Beard demonstrated that Madison designed the Constitution to prevent a collectivity of individuals, in the Lockean conception of authority, from undertaking common action as a democratic majority. The dilemma for Hamilton and Madison lay in what they had to acknowledge and what they had to arrest. The people can be the ultimate source of authority but they cannot be its final expression because they can be trusted neither to respect property nor even to preserve their own liberties, both of which had to be safeguarded by the "auxiliary precautions" of the Constitution's mechanisms.<sup>21</sup> Thus, the power of government to rule is justified by a right that it derives from the sovereign consent of the people; yet the will of the people must not prevail. Why not?

"The will of the people," according to Edmund S. Morgan, is "our own favorite fiction," a long-held mystique of collective solidarity that cannot possibly be realized in modern political institutions.<sup>22</sup> The idea may be a technical impossibility. Indeed, John Adams pointed out to French critics of the Constitution that all power and authority cannot be invested in "the nation" as the embodiment of "the people" since popular majorities do not think or act together.<sup>23</sup> But *The Federalist's* writers rejected the idea not because it was impossible but because it was undesirable. If the collective will of popular majorities were a "fiction," many of the Constitution's safeguards and the urgent warnings of *The Federalist* become inexplicable. Normally, something that cannot happen is not

<sup>18</sup> Locke, *The Second Treatise of Government*, ed. Thomas P. Peardon (Liberal Arts edn., Indianapolis, 1952), 16-30.

<sup>19</sup> *The Federalist*, no. 10.

<sup>20</sup> [Hume] *David Hume: Philosophical Historian*, ed. David F. Norton and Richard H. Popkin (Indianapolis, 1965), 20-34.

<sup>21</sup> *The Federalist*, no. 51.

<sup>22</sup> Morgan, "The Great Political Fiction," *New York Review of Books*, March 9, 1978, pp. 13-18.

<sup>23</sup> Adams, *A Defense of the Constitutions of Government of the United States of America*, 3 (1789-91; DeCapo Press edn., New York, 1971): 209-380 *passim*.



also something that need be feared. Yet the framers were haunted by the specter of a conflict between the two principles, although historians have attributed these fears not to them but to Beard. Hence Jack Greene has claimed, "By reading back into the Revolution the conflict between property rights and human rights that seemed to him so fundamental to early twentieth-century American politics, Beard and other Progressives had completely distorted and misread the eighteenth-century conception of liberty, which was always coupled with, and not in opposition to, property, and thereby built their whole account upon an anachronism."<sup>24</sup>

Granted, the eighteenth-century concept of liberty may have subsumed the idea of property during the Revolution, but it did not do so during the framing of the Constitution, at least insofar as *The Federalist* reveals the concept of liberty invoked in 1788. The eighteenth-century authors of that document, not the "Progressive" Beard, were the first to recognize and illuminate the ideological conflict between liberty and property—between the rights of popular majorities to assert their collective will and the rights of minority interests to turn to the state to oppose that will or, in essence, between the people's ability to act and the government's capacity to protect. "If a majority be united in a common interest," warned Madison in *The Federalist*, number 51, "the rights of minorities will be insecure." Madison feared a "common motive," the same "passion" and "interest" taking hold of a dominant faction as desirous of possessing property as the minority is of preserving it. To prevent that possibility and protect the rights of property, the liberty of the majority had to be checked and suspended so that its will could not be directly expressed. And that direct expression of the popular will was what the Declaration of Independence articulated a dozen years earlier.

BEARD AND THE PROGRESSIVES erroneously implied—in the terms of the second, related criticism—that the Constitution, with its tenuous balance of reason and passion, its juxtaposition of countervailing factions, and its network of checks and restraints, departed significantly from the brave, liberating spirit of the Declaration of Independence. To make that claim, Beard's critics have argued on two grounds: (1) contemporary theories of representation aimed to defend the people from despotism and political oppression, not to articulate their popular will; and (2) the Constitution's checks did not set up any barriers that would necessarily prevent the realization of the ideals of 1776.<sup>25</sup> Beard's critics are correct in pointing out that both the Declaration and the Constitution had liberty as their object, that both documents represented the views of a rough cross-section of the public and not necessarily the views of distinct classes and factions, and that both reflected broad statements of political principles and not the narrow machinations of economic interests. Indeed, the republican principle of lib-

<sup>24</sup> Greene, *The Reinterpretation of the American Revolution*, 59.

<sup>25</sup> Richard Buel, Jr., "Democracy and the American Revolution: A Frame of Reference," *WMQ*, 21 (1964): 165–90; and Martin Diamond, "Democracy and the Federalists: A Reconsideration of the Framers' Intent," *American Political Science Review*, 53 (1959): 52–68.



erty was precisely what the Constitution aspired to preserve, as Madison took pains to explain to Jefferson in 1787.<sup>26</sup> Still, despite these continuities, the historical situations reflected in the Declaration and in the Constitution required two entirely different political challenges: the Revolution aimed to resist tyranny, while the creation of the new federal government aimed to control democracy. Each situation also called for different attitudes toward the location of sovereignty and the procedure of consent. The result was that, even though the Constitution had preserved liberty, the meaning of liberty in the Declaration had been changed in the Constitution because the meaning of authority in the Constitution had become entirely different from its meaning in 1776.

The Declaration rests on the principle that popular consent is the source of the legitimate exercise of power. The Constitution rests on the assumption that, since popular majorities pose a constant threat, legitimate authority can no longer have its expression in the sovereign, active, determining will of the people. The shift from a democratic rationale for a theory of revolution to a republican rationale for a theory of government indicates that, in some respects, an older theory of consent was replaced by a newer attitude toward the role of "the people" in political life. Locke believed that the majority constituted the sovereign will through which society acts because the majority would never deprive itself of the ends for which man contracted to enter political society—namely, the preservation of life and property. Both Locke, in *The Second Treatise of Government*, and Jefferson, in the Declaration of Independence, postulated that legitimate authority arises from an act of voluntary obligation assumed by the will of all, and this presumption refers to the dissolution of a government as well as to its formation. Thus, when Jefferson declared that "it becomes necessary for one people to dissolve the political bands which have connected them with another," he extended Locke's argument that each and every person by virtue of the fundamental social contract "puts himself under an obligation to everyone of that society to submit to the determination of the majority and to be concluded by it."<sup>27</sup> By 1787, the idea of "one people" moving in a single body seems to have disappeared as rapidly as the framers abandoned Locke for Hume, who believed that the people can only unmake a government, not create it as a collective body. True, the Constitution's prologue begins with "We the people," and the ratification process met at least some criteria of popular consent; but, when Madison and Hamilton defended the draft in *The Federalist*, they turned Locke on his head by boasting that the majority would be unable to determine its own will because of "the total exclusion of the people in their collective capacity."<sup>28</sup>

Beard may have erred in treating the authors of *The Federalist* as though they were synonymous with the framers of the Constitution, and he may not have considered that the difference between 1776 and 1787 was simply the difference

<sup>26</sup> Madison to Jefferson, October 24, 1787, in Adrienne Koch, ed., *The American Enlightenment* (New York, 1965), 439–46.

<sup>27</sup> Locke, *Second Treatise of Government*, 55.

<sup>28</sup> *The Federalist*, no. 63.

between two versions of republicanism. But we ought not to be indifferent to that difference. Whether or not the new form of representation articulated in *The Federalist* was designed to protect and promote vested interests, as the Beardians have maintained, or to establish good and wise government, as the anti-Beardians have argued, this issue dramatizes one of the differences between the Declaration and the Constitution. The difference between the brave Lockeanes of 1776 and the chastened Hobbesians and Humeans of 1787, between those who were first concerned with the challenge of making government free and then with the task of making it strong, suggested to Beard that perhaps the delegates did not proceed "under the guidance of abstract principles of political science" but wanted mainly to secure the interests of the wealthy few against the threat of numerical majorities.<sup>29</sup>

This Beardian interpretation has hardly weathered the penetrating scholarship of his critics. The critics, however, cannot claim the whole truth. Beard, to be sure, failed to see what J. R. Pole and others have discovered—that the Constitution more fully represented "persons" than "property" and that the principle of majority rule developed freely because in several states urban population centers were also the centers of mercantile wealth.<sup>30</sup> But Beard's position is less anachronistic than his critics have supposed. Beard anticipated much of what Gordon S. Wood has so amply documented—that prior to the Constitutional Convention there emerged a "political pathology" based upon the fear by the few of the growing power of the many in the extra-legislative bodies in the states, and, without that apprehension, how the Federalists "conceived of the Constitution as a political device designed to control the social forces that the Revolution had released" cannot be explained.<sup>31</sup> The economic categories Beard used to interpret such "social forces" may be questionable in view of the variety and fluidity of property holdings in post-Revolutionary America.<sup>32</sup> But Beard was as much concerned with authority as with property, with the sovereign right of people to approve of the exercise of power over themselves. This principle, ironically, was less acceptable to those who made the Constitution than to those who later mystified it by worshipping it. Nothing seemed more absurd to Beard than the conventional wisdom of his era, which presumed government to be about people, not interests. "The Constitution proceeds from the whole people; the people are the original source of all political authority exercised under it."<sup>33</sup> By invoking this "juristic theory," critics have argued, Beard merely revealed his naiveté in accepting the romantic nonsense of George Bancroft and other patriotic historians of the nineteenth century—historians who, like Beard himself, remained innocent of the complexities of political representation and consent in eighteenth-century America.<sup>34</sup>

<sup>29</sup> Beard, *An Economic Interpretation of the Constitution*, 73.

<sup>30</sup> Pole, *Paths to the American Past* (New York, 1979), 41–54.

<sup>31</sup> Wood, *Creation of the American Republic*, 393–429, 476.

<sup>32</sup> Forrest McDonald, *We the People: The Economic Origins of the Constitution* (Chicago, 1958), esp. 3–18. But also see Jackson T. Main, "Charles A. Beard and the Constitution: A Critical Review of Forrest McDonald's *We the People*," *WMQ*, 17 (1960): 86–110.

<sup>33</sup> Beard, *An Economic Interpretation of the Constitution*, 10.

<sup>34</sup> Hofstadter, *The Progressive Historians*, 207–84.

Beard may never have examined the nuances of consent theory in the Revolutionary era, but his concern with the alienation of power from authority can hardly be regarded as a twentieth-century fixation imposed on the eighteenth-century political mind. The Revolution's doctrine of natural rights granted to man not only the right to property and liberty but also the "duty" to preserve his freedom, not to alienate his rights by transferring them to others, and to acknowledge no power that does not issue from himself.<sup>35</sup> The simplicity, even the innocence, of this theory of active consent was complicated—and, perhaps, rendered obsolete—by the Federalists' demands for new modes of representation in the Constitution. Still, since Beard was as much concerned as any eighteenth-century thinker about the fate of liberty conceived as a natural right that, theoretically, cannot be alienated or renounced, the right not to be subjected to someone else's power without consent,<sup>36</sup> his interpretation cannot be regarded simply as the imposition of modern distinctions on colonial thought.

Thus Beard was entirely correct in making a distinction between the Declaration and the Constitution, for, although the former document grounds rights in moral principles, the latter does not refer to any legitimating principles that have their basis in ideas as "sacred" and "undeniable" truths, ideas that are not contingent upon the purposes and interests of men in a specific historical context but, instead, are intrinsically true because of their transcendent nature. To say that the Declaration is about natural rights and liberties and the Constitution about power and interests may be an oversimplification. But in *The Federalist* Hamilton and Madison avoided articulating the social contract theory and, therefore, did not explicitly specify where man derives his right to have rights.<sup>37</sup> Man simply possesses them to the extent that society recognizes them; and the test of rights is whether or not they can be exercised, whether man has the means to give political effect to them. Rights, like property, are an aspect of power, more a social possession than a natural endowment, and they can be safeguarded in the Constitution not by appealing to "self-evident" truths but by dispersing and countervailing power in order to control it. By 1788, the doctrine of rights had a political status that no longer required an ontological foundation in the laws of nature and of God. To put it another way, the Declaration had its justification in knowledge that could be derived from nature, the Constitution in truths that reside in power.<sup>38</sup>

<sup>35</sup> Morton White, *The Philosophy of the American Revolution* (New York, 1978), esp. 229–56.

<sup>36</sup> Perhaps theories of consent were also in a state of flux during the period between the Revolution and the Constitution. For some thinkers, consent implied a sustained participation in the formulation of the rules and laws that govern society. *The Federalists's* authors, however, provided for the original consent of the people in ratifying the Constitution but restricted the continuous, active involvement of "the people"—restricted their "collective capacity"—in government.

<sup>37</sup> There is, however, a passing reference to "natural rights" in John Jay, *The Federalist*, no. 2.

<sup>38</sup> Possibly, Hume's influence led *The Federalist's* authors to develop a theory of political authority that did not resort to such familiar Lockean principles as "reason" and "natural rights," concepts that served to sanction conduct. Stated another way: in Locke, liberty and property are almost synonymous insofar as all men have a "natural" right to self-preservation, and property as the fruit of labor is a means of preserving life and, hence, deserves the protection of government. In Hume, however, liberty is functional and property conditional; both are not rights that are good in and of themselves, "natural rights" based on a conception of the priority of individual liberty to social authority. Although the philosophies of Locke and Hume lead to the same conclusion—namely, that property and liberty are essential to society's survival and must, therefore, be

BEARD'S PREOCCUPATION WITH THE CONFLICT between liberty and property and the distinction between the Declaration and the Constitution has been attributed to his inattention to eighteenth-century ideas. The third criticism follows a similar indictment and involves his unfamiliarity with Whig political thought. Had Beard steeped himself in eighteenth-century ideology, he would have seen that possession of property endows man with political "virtue" and a spirit of economic "independence" essential to the exercise of true liberty. Had he possessed, moreover, a sensibility to the culture of class relations of the era, he would have appreciated the role of social "deference" that led the mass of people to accept the leadership of propertied elites. Beard's failure, lamented Hofstadter, lay in his understanding the meaning of property from the stance of "Progressives in dealing with large business corporations" rather than from the viewpoint of eighteenth-century "philosophical premises." In that era the "lack of property was associated with servility, if not with servitude." Property alone gave man the moral character to participate in a life of civic virtue. "To have political competence, it was assumed in Whig theory, a man must stand above being ordered about, or bought or bribed, and this status would be safeguarded by the possession of some property; only in this way would the voter have, it was said (following Montesquieu and Blackstone), 'a will of his own.'"<sup>39</sup>

A close examination of these "philosophical premises" suggests, however, that historians have taken the fetish of "independence" more seriously than did the Federalists.<sup>40</sup> How, one may ask, can an economic possession like property enable the citizen to "stand above" the pull of politics? When *The Federalist's* authors reminded readers that too much "independence" destroyed the republics of ancient Greece,<sup>41</sup> they made a case for a new government on the grounds that neither individuals nor their states could depend on themselves for their protection. Madison made it clear, moreover, that the logic of self-interest would make propertyholders dependent upon the factions that expressed their interests. So, too, with the nature of representative government, which could only mean that each voter, instead of possessing "a will of his own," had surrendered part of his sovereignty to elected leaders. And, if the poor lose their liberty by selling their votes, what are the rich doing all of the time? We need not accept Robert Dahl's claim that "the essence of all competitive politics is bribery of the electorate by the politicians"<sup>42</sup> to recognize what did disturb some Founders who justified property qualifications: Rousseau's observation that "to alienate is to give or sell."<sup>43</sup> On this issue it is easy, to be sure, to succumb to cynicism.

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protected by government—Hume was not concerned with the moral legitimacy of such institutions or with the historical origins of their rightful authority in what he called "sacred" and "inviolable" principles. Locke, *Second Treatise of Government*, 44–73; and [Hume] *David Hume: Philosophical Historian*, 20–74. And, as Beard noted, the American Constitution is silent on the origins and philosophy of property; *An Economic Interpretation of the Constitution*, 176.

<sup>39</sup> Hofstadter, *The Progressive Historians*, 256.

<sup>40</sup> After examining the philosophical rationale for property qualifications during the Revolutionary era, Morton White concluded that such thinking amounted to a dubious moral philosophy that "fallaciously identified poverty with metaphysical bondage"; *Philosophy of the American Revolution*, 263.

<sup>41</sup> *The Federalist*, no. 18.

<sup>42</sup> Dahl, *A Preface to Democratic Theory* (Chicago, 1956), 68.

<sup>43</sup> James Otis cited this passage from Rousseau's *Social Contract*, bk. 1, chap. 4, as quoted in White, *Philosophy of the American Revolution*, 196.

And one sad servitude alike denotes  
The slave that labours and the slave that votes.<sup>44</sup>

Yet perhaps a dose of cynicism may be necessary to question the property-independence equation that had little to do with Beard's quarrel with the Constitution or with the framers' defense of it.

That the poor would be denied political rights while the state could justify its authority and power over them is not the issue. The point is that the framers themselves recognized that real human and moral independence may have to be sacrificed to the political and economic needs of the state. After all, a nation without a nation state was, Hamilton observed, "an awful spectacle"; and, to secure the life and property of its citizens, a government, Madison advised, had to be able to oblige the governed to obey its authority.<sup>45</sup> Listening to Hamilton and Madison suggests that historians who invoke the facile Whig principle of "independence" to deny the existence of conflict are speaking a language foreign to *The Federalist*. The authors of that document operated from an assumption that challenges the neo-Whig historians of our time: man is not free and independent because his will is determined by the object of his "passions," by forces beyond his own rational self-control. Starting from this premise, Madison framed a political system based on the interdependence of each upon all others, a relation of mutual dependency that balances faction against faction to control them and organizes the branches of government by the strategy of reciprocal distrust. The very interest-bound and suspicion-rooted psychology on which Madison and Hamilton structured the Constitution presupposes that economic and political action oriented to the behavior of others can depend upon the constancy of that behavior. If men were truly "independent," if they did not react to the actions of others upon themselves because they were not competing for the same objects, if they had no interests because they had no wants, the federal system would fail for lack of conflict.

Obviously, the property-independence equation more aptly applies to Jefferson. Fearing that "dependence begets subservience and venality,"<sup>46</sup> Jefferson looked to those who austere worked the materials of earth as the paragons of liberty and virtue—admittedly, a curious view for a plantation owner who allowed his own economic life to become so dependent and debt-ridden that he was no longer free to free others. Yet the Federalists were not bound to such tender Jeffersonian sensibilities when framing the Constitution. Nor did such principles later disturb Hamilton's own peculiar paradox. Hamilton needed a class that was sufficiently wealthy to be independent of the income that political power might bring to it yet a class that could see that its interests and those of the nation were bound in an "intimate connexion."<sup>47</sup> Hamilton realized that the demands of property do not stimulate the spirit of independence but, instead,

<sup>44</sup> Peter Pindar, as quoted in Bertrand de Jouvenal, *On Power: Its Nature and the History of Its Growth* (1945; Boston, 1962), 318.

<sup>45</sup> *The Federalist*, nos. 51, 85.

<sup>46</sup> [Jefferson] *The Papers of Thomas Jefferson*, ed. Julian P. Boyd, 8 (Princeton, 1950): 426.

<sup>47</sup> See John R. Nelson, "Alexander Hamilton and American Manufacturing: A Reexamination," *Journal of American History*, 65 (1979): 971-95.



establish the conditions for order and stability by virtue of the individual's increasing loss of autonomy and the inevitable integration of economy and society. No doubt Madison may have been disturbed by Hamilton's proposal to make the government dependent upon the rich in order to make the rich dependent upon the government. Yet Madison was also preoccupied with the problem of power as well as liberty, and he, too, could scarcely see the individual apart from the political associations through which an individual's interests are identified. If independence and individual liberty refer to the ability to act freely under the rule of law, power is the ability to act in concert regardless of the law, and it was the problem of this "collective capacity" that Madison pondered with such brilliance; in *The Federalist*, number 33, he defined power as the "ability or faculty of doing a thing," the capacity, that is, of a government or a people carrying out its will. Insofar as economic interests find expression in political power and power is not the attribute of individuals but of groups and the interaction of factions, Madison and Hamilton could hardly be expected to see "independence" as the fount of liberty or property. Nor could Beard.

Beard's unfamiliarity with Whig premises with respect to "independence" has a corollary in the argument for "deference." According to his critics, Beard could not see the political reality of the eighteenth century because his twentieth-century democratic sympathies blinded him from grasping the meaning of a "deferential society."<sup>48</sup> This argument is fraught with ironies, not the least of which is the possibility that as much deference was extended to the hegemony of wealth and power in late-nineteenth-century industrial America as in colonial America.<sup>49</sup> A more serious irony, however, lies in the basic incompatibility of the reality of deference and the concept of independence. Not only does deference reinforce the dependency of inferiors upon superiors, but it also requires people to forsake their own liberty to think and act for themselves in order to identify with those who hold power and enjoy prestige. On this disturbing truth disparate minds could agree. Thorstein Veblen tried to tell Americans in Beard's era how this psychology worked to reduce citizens to a state of false consciousness; John Adams tried to tell Americans of Jefferson's era why deference worked against the liberation of consciousness itself. There could be no distinction between a "natural" and an "artificial" aristocracy, Adams reminded Jefferson, because the human tendency to "emulate" bewitched the mind into false identifications. Given Adams's reservations about the possibility of aristocratic leadership, it is curious that Beard's critics have not been content to explain the phenomenon of deference; instead, they have had to defend it. Yet on this issue Adams seems closer to the skepticism of Veblen and Beard than to the neo-Whig scholars, which suggests that one need not be an eighteenth-century historian to perceive a general truth about human behavior.<sup>50</sup>

<sup>48</sup> J. R. Pole, "Historians and the Problem of Early American Democracy," *AHR*, 67 (1961-62): 626-46; and Buel, "Democracy and the American Revolution," 165-90.

<sup>49</sup> For a comparison of John Adams, Benjamin Franklin, and Thorstein Veblen on the implications of deference, emulation, and hegemony, see my *The Bard of Savagery: Thorstein Veblen and Modern Social Theory* (New York, 1978), 119-33.

<sup>50</sup> John Adams to Jefferson, November 15, 1813, in Koch, *The American Enlightenment*, 218-20; and, on Adams, Veblen, and the social-psychological determinants of emulation and deference, see Arthur O. Lovejoy, *Reflections on Human Nature* (Baltimore, 1961), 197-215.



With characteristic prescience, Adams perceived a twentieth-century sociological insight that historians of the eighteenth century seem to have overlooked: the impossibility of eliminating subjective needs in objective relationships of domination and subordination makes moral leadership problematic and political liberty precarious. The narrowly conceived political theory of deference used by Beard's critics remains incomplete without a social-psychological theory of submission that makes the phenomenon possible.<sup>51</sup> Jean-Jacques Rousseau, Denis Diderot, and other Enlightenment thinkers, for example, warned that deference meant the loss of liberty insofar as that subordination entailed the alienation of the political self. Even more to the point, Adams worried that the very deference that he believed was vital to social order could also mean, given men's emulative nature, a form of "slavery," a society in which "men of all sorts are chained down to an incessant servitude to their fellow creatures."<sup>52</sup> If, by accepting deference, "the great masses of people" also accepted the rule of a social or economic elite as beneficial to mass interests (the argument of Richard Buel and J. R. Pole),<sup>53</sup> did they necessarily defer to those who would rule in their interests? *The Federalist's* writers occasionally expressed fear that the leaders would "betray" the led,<sup>54</sup> and Adams elevated this apprehension into an axiom that casts doubt on the reasoning of Beard's neo-Whig critics.

The problem of deference preoccupied Adams's thoughts during the period of the Constitution, perhaps because he recognized that a deferential society requires a firmly structured social order, the kind of order that Adams sought but ultimately did not find. He saw that deference has little to do with liberty, independence, and virtue and much to do with property, power, and prestige, with those base "passions" that rendered questionable a harmony of interests between leaders and followers in the eighteenth century.<sup>55</sup> Although Adams remained suspicious of deference because disinterested behavior directed toward the public good was beyond his imagination, others recognized a truth that made Adams's dream of an aristocracy of virtue a lost hope—America was not a traditional deferential society with persisting status groups. Only in a democracy does deference become a problem to established elites, for as freedom devolves from state to society emulation can no longer be assumed to function in familiar ways as the rules of deference change. Thus in America, as *The Federalist's* authors discerned, the allocation of status was insufficiently stable to allow for the predominance of hierarchal stratification by deference. Nor were older

<sup>51</sup> J. G. A. Pocock, "The Classical Theory of Deference," *AHR*, 81 (1976): 516–23. In an attempt to refute the Lockean emphasis of both Beard and Louis Hartz, Pocock extended such classical ideas as deference and virtue to post-Revolutionary America; see Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975), 462–562. The embarrassing irony, however, is that, although the "civic humanism" of the Machiavellian tradition was designed to create and preserve a republic, that tradition was neither what the leading framers drew upon to create the American republic nor what Lincoln invoked to preserve it. I am currently working on a paper on "Classical Republicanism, Civic Humanism, and the Problem of Authority."

<sup>52</sup> Adams, *Discourses on Davila* (1790; DeCapo Press edn., New York, 1973), 44.

<sup>53</sup> Pole, "Historians and the Problem of Early American Democracy," 626–45; and Buel, "Democracy and the American Revolution," 165–90.

<sup>54</sup> *The Federalist*, no. 63.

<sup>55</sup> Adams, *Discourses on Davila*, 25–54; and, on Beard's admiration for Adams, see his *Economic Origins of Jeffersonian Democracy*, 299–321.

forms of deference needed in the “new science of politics.” In *The Federalist*, number 53, Madison defended the older classical argument for superior men of “wisdom” and “virtue” to represent the people; but he not only reassured opponents of the Constitution that such leaders could be controlled through periodic elections, he himself denied, in *The Federalist*, number 10, that the cause of good government could depend on the presence of good men: “Enlightened statesmen will not always be at the helm.” What the framers had to recognize was that emulation is determined not by the object that commands it but by the subject that grants it, by the whims of public opinion and not the wishes of ruling elites. Since “all governments rest on opinion,” as Madison put it (drawing on Hume), the new republic had to find a stable foundation to steady the shifting opinions of the masses—a foundation rooted in the needs of the many, not the qualities of the few. “Opinions” can lead the people astray, as can their “representatives”; but “people can never willfully betray their own interests.”<sup>56</sup> To defer is to believe not in something but in somebody, to give obedience not to an axiom but to a person. In *The Federalist*, however, “deference”—compliance—is owed to constituted rules and procedures that safeguard interests and liberty, and the right of the federal system to dominate, to exercise its power and authority, is legitimated by law and the “machinery of government,” not by the superior claims of political leaders.<sup>57</sup>

Although the argument from deference was presumably intended to illuminate Beard’s anachronisms, it sheds little light on Madison’s apprehensions. Madison and Hamilton saw the majority not so much deferring to the superior wisdom of the few as envying its possessions and threatening its position, while Adams, more sensitive to the social psychology of class relations, believed that well-instilled emulation based on the work ethic could make the envy prevent the threat.<sup>58</sup> Whatever their apprehensions, the Federalists did not count upon the moral inspiration of “independence” or the unpredictable submission of “deference” to preserve the republic. Did they, instead, count upon political ideas rather than social habits?

BECAUSE BEARD LACKED SOPHISTICATION in his use of the terms “ideas” and “interests,” the fourth criticism alleges, he misunderstood the role of political ideology in the framing of the Constitution. According to one school of thought, the body of English political doctrine upon which the framers drew made liberty, power, and virtue, not property and interests, the animating principles of human action; according to the other, the Founders acted upon the American environment with no political ideas in their heads. These two approaches, the affirmation of “ideology” by Bernard Bailyn and its denial by Daniel J. Boorstin, may contribute little to refuting Beard’s thesis on the Constitution, and both tend to demonstrate the confusion engendered by the problem of dealing with

<sup>56</sup> *The Federalist*, no. 63.

<sup>57</sup> *Ibid.*, no. 10.

<sup>58</sup> [Adams] *The Political Writings of John Adams*, ed. George Peek (Indianapolis, 1954), 148–49.

political ideas in relation to economic interests in post-Beardian scholarship. Bailyn has conceded that some of the ideas of "classical republicanism" did not survive the aftermath of the Revolution to be incorporated into the Constitution, while Boorstin has argued that ideas themselves, those abstract "ideologies" that have supposedly beguiled the European political imagination, could not survive the healthy American environment itself.<sup>59</sup> Curiously, whereas Bailyn has criticized Beard and the Progressives for not paying proper attention to ideas, Boorstin has denigrated not only the Beardians but all historians for refusing to recognize that ideas do not explain America and that, indeed, the lack of reliance on ideologies has always been our saving "genius."

Hofstadter's *The Progressive Historians* devotes considerable space to exposing Beard's presumably muddled terminology on this issue. "It is easy to accept the general proposition that ideas and interests are somehow associated, easy to see that there might still have been some point in asserting it in 1913. But there are some dangers in working with any such formula." Among the "dangers" Hofstadter presented are the difficulties involved in discerning the point at which ideas leave off and interests begin, in judging the degree to which even interests are defined by ideas, in comprehending the ways in which ideas also make claims of their own that must be met, and in gauging the extent to which other "moral" and "intellectual" sentiments and aspirations influence political thought—all of which are essential to preventing the dissolution of the distinction between ideas and interests, leaving us "only with interests on our hands." The historian must keep the distinction acute, Hofstadter claimed, to avoid the pitfalls of reductionism, and this Beard failed to do. "With ideas, with moral impulses, with cultural forces that could not closely be tied to economic origins, Beard throughout his career was often quite inept."<sup>60</sup>

Like many other critics, Hofstadter discussed ideas and interests in the abstract with little reference to their possible meanings (albeit under a different vocabulary) in the actual framing of the Constitution. But *The Federalist's* authors accepted the association of ideas and interests that Beard's critics have regarded as one of the "dangers" of thinking like early-twentieth-century Progressives, and the framers did so because they presupposed a motivational commitment to the norm of interests on the part of all classes and factions regardless of the ideas held. The authors agreed that, although any individual's interests were inseparable from those of society, individuals may be incapable of thinking beyond their respective factions and responding to ideas that might instill a notion of the public good. Invoking a reverence for the idea of law might be possible, Madison noted, providing America was "a nation of philosophers." And instilling acceptance of the idea of a national government might work, Hamilton observed, if it were not that a "government continually at a distance and out of sight can hardly be expected to interest the sensations of the people."<sup>61</sup>

<sup>59</sup> Bailyn, *Ideological Origins of the American Revolution*, 272–319; and Daniel J. Boorstin, *The Genius of American Politics* (Chicago, 1953), esp. 8–36.

<sup>60</sup> Hofstadter, *The Progressive Historians*, 244–45.

<sup>61</sup> *The Federalist*, nos. 49, 27.

The distinction between ideas and interests used by both Beard and his critics may lack the conceptual and linguistic precision to satisfy intellectual historians, and it may be fraught with reductionist pitfalls; nevertheless, it has a rough counterpart in the eighteenth-century distinction between rationalism and empiricism. Does man respond to the abstract propositions of reason or to the immediate facts of experience?

"Man," wrote Hamilton, "is very much a creature of habit. A thing that rarely strikes his senses will generally have but a transient influence upon his mind."<sup>62</sup> Madison and Hamilton were convinced that ideas could not compel man's mind unless such ideas reflected the interests men were inclined to obey. Although *The Federalist's* authors spoke of the "maxims of geometry" and other deductive axioms, in developing their "new science of politics" they could no longer rely upon the efficacy of "self-evident" truths—statements and declarations that by virtue of their sheer objective truthfulness could impose themselves on a rational mind capable of responding to ideas apart from the interests that touch the senses. The Constitution, unlike the Declaration, does not rest upon the ultimate authority of ideas but upon the immediate reality of interests.<sup>63</sup>

Beard, it must be said, bears some of the responsibility for misleading a generation of scholars because he claimed that the Founders acted from economic interests rather than from constitutional principles. That unfortunate claim cannot, however, be regarded simply as an "anachronistic" Progressive interpretation, for the same charge was leveled by the Tories against the Founders at the time of the Revolution.<sup>64</sup> Siding with the Founders, Beard's critics have implied that, because the framers drew upon eighteenth-century ideas regarding the nature of man and of government, they affirmed the vital role of ideas in history, what Douglass G. Adair has called the "precepts of wisdom and reason."<sup>65</sup> Thus Bailyn proposed—after praising the "demolition" of Beardianism—to demonstrate how the Founders "took ideas seriously" and used them not as an "instrument of a particular social group" but as vehicles of "idealization" and "symbolization" that helped bring to consciousness the "inchoate" nature of historical events and thereby rendered political experience comprehensible. Also lamenting Beard's enduring influence and his "simple and uncritical commitment to a theory of economic determinism," Cecelia M. Kenyon argued that the establishment of a republican form of government in 1788 derived from the creative role of ideas and beliefs, from a "radical" and "powerful

<sup>62</sup> *Ibid.*, no. 27.

<sup>63</sup> In citing the "maxims of geometry," Hamilton and Madison did indicate that they believed in self-evident propositions, "certain primary truths, or first principles, upon which all subsequent reasoning must depend." Both authors then proceeded to demonstrate that the masses of men cannot so reason because of their "passions and prejudices" and because of the "obscurity" of language as "the vehicle of ideas." Given these epistemological impediments, the meaning of political authority had to reach man through his "sensations" and "common interests." Obligations to obedience, in short, must be based on "interests," not on "ideas." See *The Federalist*, nos. 31, 37.

<sup>64</sup> Gordon S. Wood has made this point in questioning the neo-Whig scholarship as something of an apologia for the Revolution; Wood, "Rhetoric and Reality in the American Revolution," *WMQ*, 23 (1966): 3–32.

<sup>65</sup> Adair, "That Politics May Be Reduced to a Science: David Hume, James Madison, and the Tenth Federalist," *Huntington Library Quarterly*, 20 (1957): 343–63.

ideological force at odds with our alleged pragmatism." And Edmund S. Morgan cited the writings of Harrington, Locke, and Montesquieu as evidence that the Founders were steeped in Enlightenment thought and guided by ideas rather than by interests.<sup>66</sup> To be true to the context of the Constitution, what the framers did with ideas needs to be distinguished from what they thought ideas themselves could do. A Beardian could easily concede that the framers used ideas but only to show that ideas are not useful in constructing a government. In what sense, then, were they using ideas? The criticism under discussion, Beard's dangerous association of ideas and interests, may be clarified by mentioning the various ways in which ideas can be used.

Clearly, the authors of *The Federalist* were not using ideas as moral imperatives. As Arthur O. Lovejoy put it so succinctly, the framers were concerned with predicting what men would do, not what they "ought" to do.<sup>67</sup> Nor did the framers use ideas as normative constraints based on beliefs that would govern behavior; for beliefs, the traditional domain of moral sentiment, could not be counted upon to control power. Similarly, the symbolic notion of ideas as powerful stimulants that mediate between action and experience may have been useful during the Revolutionary crisis, when fears and anxieties about "power" and "tyranny" could be acted on and, in the process of mobilizing "inchoate" emotions, translated into political ideals; but, for a theory of constitutional government, the framers needed to find ways not to excite but to discipline man's passional nature. To do so they drew upon ideas as social theory and cited the thoughts and reflections of classical political philosophers who themselves believed that ideas could explain behavior but not necessarily motivate it. Here distinguishing between ideas that are about other ideas—that is, ideas that are the images through which thought operates—and ideas that are assertions and propositions about interests that cannot be governed by ideas is essential. Beard's misfortune was to create the impression that the framers exploited ideas as rationalizations that distort the reality they describe as a means of legitimating their own interests. But the challenge of Beard's critics is to demonstrate that the framers regarded ideas as distinct from interests, as principles and ideals capable of controlling man's interests and passions.

The challenge may be posed by a simple question: Did ideas determine the nature of action, or did action determine the meaning of ideas? More specifically, did ideas about liberty determine behavior in relation to property, or did action taken in relation to property determine ideas about liberty? Such a formulation may seem a false dichotomy insofar as the concepts tended to fuse in the eighteenth-century mentality, but historians cannot refute Beard without facing this question directly. In the first instance, behavior is determined by ideas, and ideas are assumed to exist as an inherent quality of thought that can

<sup>66</sup> Bailyn, "Political Experience and Enlightenment Ideas in Eighteenth-Century America," *AHR*, 67 (1961-62): 153-82; Kenyon, "Republicanism and Radicalism in the American Revolution: An Old-Fashioned Interpretation," *WMQ*, 19 (1962): 153-82; and Morgan, "The American Revolution Considered as an Intellectual Movement," in Morton White and Arthur Schlesinger, jr., eds., *Paths of American Thought* (Boston, 1973), 11-33.

<sup>67</sup> Lovejoy, *Reflections on Human Nature*, 46.



impose obligation on conduct; in the second instance, behavior is explained by ideas, and ideas are assumed to function as a means of rendering experience meaningful. Beard wrestled continuously with the problem of whether an idea preceded a fact or vice versa, only to confuse "idea" and "interest" for thought and action, word and deed, theory and practice, and other, perhaps unresolvable dualisms. Beard may have gotten himself into a "philosophical problem," one for which, as Isaiah Berlin so aptly put it, the historian does not know the answer and does not know where to find the answer. Most of Beard's critics, however, have never missed an opportunity to demonstrate that the framers were men of ideas because they cited the works of European political thinkers, and intellectual historians assume they can overcome Beard's dualisms by employing the analytic methods of language philosophy—a dubious assumption that avoids the whole issue of causation.<sup>68</sup>

<sup>68</sup> Before settling down to an investigation of a particular historical problem, Beard advised, we as historians should consider "a closely related philosophical question which has accompanied speculation since the days of the Greeks, namely, which is older, fact or idea? 'In the beginning was the Deed' (*Am Anfang war die That*), we are informed by the poet Goethe. 'In the beginning was the Word,' we are told by the theologian. The great debate has never been closed to the satisfaction of the contestants, but William James has given us a fair working formula in the declaration that the worlds of fact and idea evolve together. Their relations are reciprocal and a sword of reason has not yet been found with an edge fine enough to separate them. Whoever seeks to penetrate to the heart of an age . . . must reckon with this instrument of thought." Beard, "Individualism and Capitalism," in Edwin R. A. Seligman, ed., *Encyclopedia of the Social Sciences*, 1 (New York, 1930): 145. Many scholars who invoke ideas to refute the economic interpretation of history have yet to discover the "sword of reason" that eluded Beard. Faced with the old Goethian dilemma of whether "the deed" follows from "the word" or precedes it, whether action derives from ideas or determines them, intellectual historians of the neo-Whig and classical persuasions resolve the dilemma simply by assuming that the word is the deed. Hence they advise us that the proper study of the history of ideas is the study of the "linguistic paradigms" through which ideas are expressed, the magical "context" that supposedly will unlock the mystery that bewildered Goethe. "The historian's first problem," advised J. G. A. Pocock, "is to identify the 'language' or 'vocabulary' with and within which the author operated, and to show how it functioned paradigmatically to prescribe what he might say and how he might say it." The advice is based on the assumption that all thinking is language-bound and determined by the social context in which thought is uttered. "Men think," Pocock observed, "by communicating language systems; these systems help constitute both their conceptual worlds and the authority-structures, or social worlds, related to these; the conceptual and social worlds may each be seen as a context to the other, so that the picture gains in concreteness. The individual's thinking may now be viewed as a social event, an act of communication and of response within a paradigm-system, and as a historical event, a moment in a process of transformation of that system and of the interacting worlds which both system and act help to constitute and are constituted by. We have gained what we lacked before: the complexity of context which the historians needs." Pocock, *Politics, Language, and Time: Essays on Political Thought and History* (New York, 1973), 15, 25. This Wittgensteinian formula by no means resolves the question of causation; indeed, it seems only to substitute one form of determinism for another, linguistic for economic. Even language philosophers concede that the study of linguistic terms in a given historical context can only tell us how language was used, not necessarily why it was used in that way. The recent tendency not only among social historians but also among intellectual historians (who should know better) has been to shy away from such intractable problems as causation and motivation and seek a solution to them by adopting structuralism, semiotics, and other modes of analysis that often confuse description for explanation. The problems Beard raised regarding ideas and behavior, however, are so important that they have never been solved, and the latest fads of structural sociology, cultural anthropology, and linguistic philosophy may offer the historian not so much a conscious solution as a systematic evasion of those problems. See John P. Diggins, "The Socialization of Authority and the Dilemmas of American Liberalism," *Social Research*, 46 (1979): 454-86; Ronald G. Walters, "Signs of the Times: Clifford Geertz and Historians," *ibid.*, 47 (1980): 537-56; and Ernest Gellner, *Words and Things: A Critical Account of Linguistic Philosophy and a Study in Ideology* (Boston, 1959), 17-56, 193-265. Even so thoughtful a historian as Gordon S. Wood, who has praised Beard's *Economic Interpretation of the Constitution* as "surely the most provocative work of history ever written in America," has recently suggested that historians should avoid Beard's dilemma and regard ideas as simply functional rather than trying to establish whether they either cause behavior or legitimate it: "Only by rejecting the futile dichotomy of ideas or beliefs as causes or effects of social forces can intellectual historians escape from what will always be a losing struggle with the realists and materialists": Wood, "Heroics," *New York Review of Books*, April 2, 1981, p. 18, and "Intellectual History and

Beard, to be sure, minimized the intellectual orientation of the framers in *An Economic Interpretation of the Constitution*, an oversight he hastened to correct in *The Economic Basis of Politics* (1922), a set of essays written between 1916 and 1922. Yet, in citing the philosophers who may have influenced the framers, Beard did not have to concede his original argument. James Harrington, Machiavelli, Locke, and Hume can hardly be regarded as philosophers who separated interests from ideas; nor did they see a constitution as being formed by purely theoretical deliberation and copied directly from an idea inside the head of a theorist, "planned in his closet or in his imagination," as Madison put it.<sup>69</sup> On the contrary, the thinkers whom the framers read agreed that the forms of government derived not from ideas but from the economic pursuits of men and the social relations of classes, and Beard fully recognized the materialist heritage of these constitutive beliefs.<sup>70</sup> The body of ideas that the framers drew upon were ideas that explained behavior; they were not ideas that claimed to have the capacity to influence behavior either by mobilizing it or restraining it. If ideas could illuminate interests, if the framers could quote Machiavelli on power or Harrington on property, it was only because interests required representation in the form of ideas. The Beardian critics who wish to distinguish ideas from interests in order to repudiate the economic basis of politics must not only consider that the two concepts were almost identical in eighteenth-century thought but also recognize that in *The Federalist* interests provided the basic motive to political obedience. In this respect, the Constitution is, as Beard maintained, an "economic document," and one with profoundly deterministic overtones.

BEARD'S "SIMPLE AND UNCRITICAL COMMITMENT to a theory of economic determinism," criticism number five, has haunted his reputation like an unpardonable sin. It is time to unburden him of it. One of the assumptions of determinism is the view that events that have happened had to happen by reason of the very description of their occurrence. This view scarcely applies to Beard, who denied that the Constitution had to happen any more than did the attack on Pearl Harbor. Yet the most frequent criticism of Beard has to do with his alleged "simple" economic determinism and his stubborn attempt to read his own predilections about the economic determinants of politics into the behavior of the framers.

Robert Brown, for example, has claimed that Madison did not see society di-

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the Social Sciences," in John Higham and Paul K. Conkin, eds., *New Directions in American Intellectual History* (Baltimore, 1979), 34. In view of such methodological advice, perhaps it is instructive to remember that the framers, who were nothing if not men of ideas and who knew, thanks to Hume, that the "struggle with the realists and the materialists" had to be overcome, faced directly many of the issues that contemporary historians like to avoid. Had the framers given up on the problem of causation and motivation, they could not have written a constitution that is capable of controlling human behavior by virtue of their ability to understand it. For a fuller discussion of this issue, see my "The Oyster and the Pearl: The Problem of Contextualism in Intellectual History," *History and Theory* (forthcoming).

<sup>69</sup> *The Federalist*, no. 37.

<sup>70</sup> Beard, "The Doctrine of the Philosophers," in his *The Economic Basis of Politics and Other Essays*, ed. William Beard (1922; Vintage edn., New York, 1957), 23-43.

vided into simple class divisions based on property. Paraphrasing Madison to stress a section of *The Federalist* that Beard omitted from his discussion, Brown has called attention to noneconomic factors. "Unfriendly passions and violent conflicts among men have been caused by zeal for different opinions on religion, government, and other things, by attachment to different leaders or others, and even by the most frivolous and fanciful distinctions."<sup>71</sup> The questions that concerned Madison the most, however, were not what divides society but what can hold society together, not what causes behavior but what controls it. With these questions uppermost, the framers concluded that they could not count upon ideas or upon any sources of authority that appealed to mind or conscience. To the extent that Madison counted upon self-interest as the mainspring of human action and to the extent that he constructed an entire edifice of political devices on this single premise, the question to ask is not whether Beard was an economic determinist but whether Madison was willing to rest the future of liberty on foundations that seem to deny the free will of men.<sup>72</sup>

Beard's critics have been determined to label him an "economic determinist" as though a serious argument can be annihilated by a simple epithet. In contrast to neo-Whig scholars, however, the Marxist historian Eugene D. Genovese respected Beard too much to dismiss him as a vulgar determinist, even though the Gramsci-struck scholar has always been troubled by Beard's "reductionist" tendency to translate every issue into a question of "economic interests."<sup>73</sup> More questionable is Lee Benson's charge that Beard revealed his confusions as well as his inconsistencies when he interchanged the terms "economic interpretation" and "economic determinism." Beard's difficulty, it seems, was that he chose not to fall back on reductionism and, therefore, could not make up his mind whether the framers were "egoists working solely or primarily for their own benefit" or "wise realists" who recognized that only economic interests could provide the solid foundations of government.<sup>74</sup> One might add, to use Max Weber's distinctions, that Beard could not say precisely whether the framers' behavior was "conditioned" by, "dependent" on, or "oriented" toward the views of their own class and whether those views actually conflicted with the interests of other classes.<sup>75</sup> But these varieties of determinism may not go to the heart of Beard's ultimate concerns.

Although Beard doubtless focused on economic factors and used his terms carelessly and although he approached the study of history as the movement of classes and factions, he started from the premise of moral choice and individual

<sup>71</sup> Brown, *Charles Beard and the Constitution: A Critical Analysis of "An Economic Interpretation of the Constitution"* (Princeton, 1956), 29.

<sup>72</sup> Whether or not it is accurate to label Madison an "economic determinist," he clearly believed that the desire for property constituted "the most common and durable" motive, which reflected the very wants that give rise to factions and, hence, that "passion" determines man's behavior, whatever may be the deeper social-psychological roots of such propensities; *The Federalist*, no. 10.

<sup>73</sup> Genovese, "Charles Beard and the Economic Interpretation of History," in Marvin C. Swanson, ed., *Charles A. Beard, Proceedings of the Beard Centennial*, DePauw University (Greencastle, Ind., 1976), 22-44.

<sup>74</sup> Benson, *Turner and Beard: American Historical Writings Reconsidered* (New York, 1960), 107-09.

<sup>75</sup> Weber, *Economy and Society: An Outline of Interpretive Sociology*, ed. Gunther Roth and Claus Wittich, 1 (Berkeley and Los Angeles, 1978): 3-74.

freedom. Insofar as he concerned himself primarily with the motivations of human conduct, with "the direct, impelling motive" of the framers,<sup>76</sup> Beard saw in the decisions of individual leaders the origins of social actions that may be expressed in class behavior even if not determined by it. Thus, for Beard, class was not an objective structure whose agents were the bearers of its "logic" regardless of their intentions. Instead, Beard always approached class in terms of the economic groups of which it was composed, and the behavior of those groups, whether Federalists or Anti-Federalists, were in turn explainable by the thought and conduct of their individual members. Using pioneering prosopographical methods, Beard suggested that faction or class behavior cannot be studied independently of the biographies of the group's component individuals. Had Beard subscribed completely to the doctrine of economic determinism, he would have felt no obligation to analyze the thoughts and lives of the individual Federalists and their opponents, as he did in *Economic Origins of Jeffersonian Democracy*; nor would he have seen any need to examine alphabetically the doctrines of the members of the Constitutional Convention or to quote at length from Madison's *The Federalist*, as he did in *An Economic Interpretation of the Constitution* in order to allow the architect of America's federal government to speak for himself.

What distinguishes Beard's "economic interpretation" from simple "economic determinism" is his effort to explain the Constitution through *The Federalist*—that is, by reference to the framers' conceptions of what they were doing—as opposed to explaining it by causes independent of their intentions or by motives so shameful that they had to be concealed. Marxists, for example, do not necessarily have to use the ideas of historical participants in developing accounts of historical events. That interpreting history need not require interpreting the motives of human action may perhaps explain why Marxists seem more comfortable with social than intellectual history and why history "from the bottom up," instead of the history of ideas, provides an opportunity to penetrate collective consciousness by reference to the objective character of class structures and economic forces. While this external dimension remained important to Beard, he also wanted to grasp "the true inwardness of the Constitution," what was being experienced within the consciousness of the framers as conveyed by the ideas and theories they used, what he later referred to as the "meaning and intention" of their written thoughts.<sup>77</sup> And he felt obligated to cite the framers' positive attitudes toward property and their negative attitudes toward democracy to indicate that their actions were conscious, deliberate, willful. Thus the claim that Beard merely sought to "unmask" the Founding Fathers, to treat them as though they were performing deeds for pronounced reasons that had little to do with their actual motives, is not entirely accurate. Perhaps the patriotic wisdom of nineteenth-century historiography could place liberty ahead of property; but for the most part Beard saw the Constitution as the result of the reasons the framers themselves gave for drafting it: the need to protect property

<sup>76</sup> Beard, *An Economic Interpretation of the Constitution*, 17–18.

<sup>77</sup> Beard, *Jefferson, Corporations, and the Constitution*, 87, and *An Economic Interpretation of the Constitution*, 152–53.

in order to preserve liberty and to reconcile "private rights" with "the spirit and form of popular government."<sup>78</sup> Where then lies the problem?

It lies, I believe, in the awkward fact that Beard departed from both Marxist and neo-Whig historians by insisting that the framers were free to act in ways other than they did as political leaders. That "power follows property" may be a dictum or "law" of history, but Beard wrote his "economic interpretation" precisely because he tried to resist the ultimate implications of reductionism and determinism. Beard's quarrel with American history began with his rejection of the assumption that the Constitution must be accepted as a necessary "stage" in America's historical development. Thus his efforts to evaluate the ideology of *The Federalist* not in terms of its profound theoretical validity but in terms of the complex motives that lay behind it had a larger purpose. Only by trying to unravel original intentions could Beard make the case that human action does not necessarily have to follow the logic of property, that the inward resources of mind remain potentially free to resist the forces of history, and that "ideas" may, therefore, be juxtaposed to "interests" if the authority of mind is ever to answer the power of wealth.

Beard realized, of course, that the framers made no such juxtaposition, but ultimately his purpose was not so much to blame them for acting upon considerations of power and property as to expose later historians for bowing before the material forces of history in the name of higher ideals. It was, he said in praise of Henry Adams, the "Totemism" of the "Johns Hopkins school of Teutonism" that led to the sanctification of the Constitution and the arrogance of an American Historical Association that ignored what Adams was valiantly trying to protest in the modern world: interests without legitimacy and power without authority.<sup>79</sup> A "rebel against authority," as Hofstadter aptly described him,<sup>80</sup> Beard refused to accept the Constitution until he questioned the presumption of its historical inevitability. To do so, he attempted to explain actions by discovering motives so as to judge motives according to principles. While Marxists accept the inevitability of interests (the "Furies of private interests," in Marx's terms<sup>81</sup>) and Whigs the inevitability of property as central to the movement of history, Beard shared Henry Adams's view of interests and property not as principles of historical understanding but simply as forms of power that would come to dominate history itself. To justify allegiance to the Constitution in order to translate power into legitimate authority required, for Beard, something more than mere interests and power. It required ideas that are anterior to interests and transcend property and power, ideas that are intrinsically true and just and deserve our allegiance because of their moral authority. *The Federalist* provides no such body of transcendent ideas; and, when Beard later accepted the Constitution, as he did in *The Republic* (1943), he did so because by then he

<sup>78</sup> Beard, *Economic Basis of Politics*, 141.

<sup>79</sup> Beard, "Henry Adams," *New Republic*, 22 (1920): 162.

<sup>80</sup> Hofstadter, *The Progressive Historians*, 216.

<sup>81</sup> "[T]he most violent, mean and malignant passions of the human breast, the Furies of private interests"; Karl Marx, *Capital: A Critical Analysis of Capitalist Production*, trans. Samuel Moore and Edward Aveling, 1 (Moscow, n.d.): 21.



believed that its legitimacy need not rest on ideas or on any ultimate principles regarding liberty or natural rights.

BEARD HAD A SUFFICIENT UNDERSTANDING of *The Federalist* not to have to address himself to these five issues raised by neo-Whig historians—issues such as “independence,” “deference,” and “ideas” as legitimating beliefs for the rule of property and its harmony with liberty. Yet, if Beard and the leading framers had something of a common understanding, why did Beard question the Constitution’s authority while the framers sought to legitimate it? Any answer to this question must be formulated against the hopes of the framers and the unanticipated consequences that flowed from their handiwork.

In *The Federalist*, number 51, Madison argued that the new Constitution would prevent the development of “some power altogether independent of the people,” the great fear of the Anti-Federalists that had to be allayed. Since the system of checks and balances supposedly precludes the need to go beyond constituted government to protect minorities, there should be no occasion for the development of a political will alien to the republic, “a will independent of society itself.” In Beard’s estimation, Madison’s fear eventually materialized as economic power became more and more dissociated from the constraints of political authority and all but ruled society with a “will” of its own. The new form of business power that characterized Beard’s era did not, of course, resemble the arbitrary, despotic political power against which the Founders had fought the Revolution; nor did it resemble the mass power of democratic majorities that the Constitution sought to curb. Nevertheless, corporate power did express a phenomenon that grew out of two developments that seemed to render Madison’s system unworkable: the assumption of the right of judicial review as a form of usurpation, and the growth and consolidation of economic interests as a form of domination.

Unlike many Progressive writers, Beard did not claim that members of Constitutional Convention never intended to give the courts the power to declare laws of Congress void. But his thorough knowledge of *The Federalist*, which he quoted at length in *The Supreme Court and the Constitution* (1912), made him aware that the framers could not easily reconcile the values of law with the values of democracy in ways that would subject the dynamics of economic power to the dictates of legal authority. In *The Federalist*, number 22, which Beard also quoted, Hamilton spoke like a populist apparently in an effort to win over those who feared that the centralization of power meant the loss of local control. “The fabric of American empire ought to rest on the solid basis of the consent of the people. The streams of national power ought to flow immediately from that pure, original fountain of all legitimate authority.” In *The Federalist*, number 78, however, Hamilton explained why it is necessary to invest “the majesty of national authority” in the judiciary, a safe depository because it possessed “neither Force nor Will” and would therefore be the “weakest” of the three branches of government. Hamilton wisely sought to separate power from authority, popular

will from judicial wisdom. But Beard realized that, while authority can be communicated by law, power is imposed by those who actually make law, by the justices who remain independent of popular control. And in the nineteenth century the justices' peculiar reading of the Fifth and Fourteenth Amendments rendered the "majesty" of constitutional authority impotent to control the activity of economic power. Corporations in post-Civil War America came to enjoy the status of "persons" under the law, whose natural rights entitled them to protection from legislative interference. Thus did "a will independent of society" prevail as a right of property above and beyond the consent of the people, "that pure, original fountain of all legitimate authority."

If Hamilton's theory of the judiciary resulted in the alienation of power from authority, Madison's theory of representation resulted in the fracture of the "fountain" itself. A majority can be prevented from acting in concert, Beard quoted Madison as observing in *The Federalist*, number 51, because of the diversity of its interests in a large, "extensive republic." "Whilst all authority in it will be derived from and dependent on the society, the society itself will be broken into so many parts, interests, and classes of citizens, that the rights of individuals, or of the minority, will be in little danger from the interested combination of the majority." Such a scheme, Beard was convinced, means that the will of the majority becomes frustrated while the activities of interests that move under the sanctity of law are protected:

If we carefully examine the delicate instrument by which the framers sought to check certain kinds of positive action that might be advocated to the detriment of established and acquired rights, we cannot help marvelling at their skill. Their leading idea was to break up the attacking forces at the starting point: the source of political authority for the several branches of government. This disintegration of positive action at the source was further facilitated by the differentiation in the terms given to the respective departments of government. And the crowning counterweight to "an interested and over-bearing majority," as Madison phrased it, was secured in the peculiar position assigned to the judiciary, and the use of the sanctity and mystery of the law as a foil to democratic attacks.<sup>82</sup>

Whether or not Beard accurately interpreted the framers' intent, there can be little doubt about the unintended consequences of the ideas and premises upon which they built the Constitution. In attempting to disperse power, the framers also destroyed authority. For Madison had so fragmented the "fountain" that there remains no criterion for identifying the ultimate source of authority. Thus, while the Constitution allows for activation of power in pursuit of interests, the active and unifying force of sovereign authority not only is absent but deemed unnecessary and undesirable as well. The result is that the people are denied the "collective capacity" to act against property, while property came to be protected by a government that allows a constellation of its interests to exercise power over those who have not willed its exercise. Although one wishes that Beard had acknowledged in his early work that the framers did not intend such

<sup>82</sup> Beard, *An Economic Interpretation of the Constitution*, 161.

a development,<sup>83</sup> he nonetheless did make clear what ultimately may be a more important truth: in American history, authority and power remain alienated from one another as long as democratic majorities cannot control property through political means.

Yet Beard, it must be admitted, missed the meaning of legitimation in *The Federalist*, the reasoning by which power is justified and thereby translated into rightful authority. Hamilton and Madison explained the need for government not as an institution that protects specific interests but as a political imperative that preserves society by saving men from each other and, indeed, from themselves. This Humean theme, which has post-Calvinistic residues, helps explain how Hamilton and Madison could assume that they could convince popular majorities why popular majorities are not to be trusted. Government may have as its "first object" the protection of property-acquiring "faculties," a Madison utterance that Beard seized upon perhaps too eagerly; for this objective merely describes the function of government, not its legitimacy. The origins of the Constitution's authority, the reason why the new government had to do what needed to be done, and the reasons why subjects should obey the power that the Constitution places over them lie in a source deeper than property and more binding than economic interests. The source lies in man's natural state, the eternal human condition that calls for external controls to make up for internal defects, a political solution to a spiritual problem without which there would be no need for government to protect either man or property. "What is government itself," Madison exclaimed, "but the greatest of all reflections on human nature." "Why," asked Hamilton, "has government been instituted at all?" Why indeed should man accept the authority of the state? "Because the passions of men will not conform to the dictates of reason and justice without constraint."<sup>84</sup>

Had Beard considered that the framers, in providing explanations as to why government was necessary, were providing the reasons by which subjects derive their obligations to obey, he would perhaps have faced directly Madison's conviction that the sources of faction are "sown" within the nature of man and that the Constitution spoke not to the historical moment but to the human condition. Instead, Beard saw the Constitution as an unauthorized act, a coup d'état put over on a public that had not been fully consulted; and he jumped to the conclusion that the power exercised by the Federalists violated the authority of the people—a conclusion he could assert only because he assumed that the interests of those exercising power must contradict the interests of those who acquiesce to it. Here Beard went profoundly wrong, for a close reading of *The Federalist* indicates that Madison structured government not on the premise of irreconcilable conflict of differing classes but on the mutuality of competing factions motivated by the same interests and passions. If American history had

<sup>83</sup> Not until World War II did Beard fully acknowledge this. In *The Rise of American Civilization*, he and co-author Mary Beard held that the framers "made the new Constitution an instrument of power in the direction of national economy and in the distribution of wealth"; Beard and Beard, *The Rise of American Civilization* (New York, 1930), 338.

<sup>84</sup> *The Federalist*, nos. 15, 51.

really been ridden with class conflict, as Beard mistakenly implied, the Constitution could not have survived the various kinds of social tensions that disrupted European history. In this respect Beard's critics are correct: consensus, not conflict, explains the "uniqueness" of American history.<sup>85</sup>

Thus, any reconsideration of the man and his work ought to avoid the stale paradigms of social conflict and economic determinism. A better approach would be to see Charles A. Beard as an intellectual who happened to be a historian or, perhaps better still, as an intellectual historian who could not find the ideas he was looking for to establish a moral basis for genuine authority in America. The "search for a scheme of ethics," he wrote in 1932, must appeal to "reason and the noblest impulses of mankind." As a moralist, Beard wanted to judge history by some standard other than the relentless activity of interests and power. "What, then, is the immovable base, the fixed bench mark, from which to survey the land of values?"<sup>86</sup> Beard sensed he would never be able to resolve the dilemmas of relativism in his search for "that noble dream" of objectivity that could give the study of history authoritative status.<sup>87</sup> He therefore took his place along the side of "that profound scholar, Henry Adams,"<sup>88</sup> as a critic of power and domination in the name of a true and rightful authority that neither could discover in an American history seemingly controlled at every turn by the inexorability of interests and power. Beard's search ended where Adams's had begun: knowledge without truth means power without authority.

<sup>85</sup> Louis Hartz, *The Liberal Tradition in America* (New York, 1955), esp. 3–86, 228–325.

<sup>86</sup> Beard, "Search for the Centre," *Scribner's Magazine*, 91 (1932): 3.

<sup>87</sup> Beard, "That Noble Dream," *AHR*, 41 (1935–36): 74–87. For a recent analysis of Beard's struggle with relativism, see Ellen Nore, "Charles A. Beard's Act of Faith: Context and Content," *Journal of American History*, 66 (1980): 850–66.

<sup>88</sup> Beard returned to Henry Adams's haunting vision of history as chaos and entropy in the final passage of *The Rise of American Civilization*. Compare, for example, Beard's disturbing speculations about "Power" at the end of this great book to those of Adams in the final passages of his monumental study of the early nineteenth century: Beard and Beard, *The Rise of American Civilization*, 799–800; and Adams, *The History of the United States of America during the Administrations of Jefferson and Madison*, 9 (New York, 1891): 241–42.

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## Marxism and Russian Rural Development: Problems of Evidence, Experience, and Culture

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ESTHER KINGSTON-MANN

The common ploughman, though generally regarded as the pattern of stupidity and ignorance, is seldom defective in his judgement and discretion. He is less accustomed, indeed, to social intercourse than the mechanic who lives in a town. His voice and language are more uncouth and more difficult to be understood by those who are not used to them. His understanding, however, is generally superior to that of the other, whose whole attention from morning till night is commonly occupied in performing one or two very simple operations.

—Adam Smith, *The Wealth of Nations* (1776)<sup>1</sup>

*mir*: 1. peace; 2. world, universe; 3. village community.

IN THE TWENTIETH CENTURY, tsarist and Soviet modernizers set out to destroy the loyalties to extended family, territory, and community created by centuries of successful peasant efforts to survive traditional forms of exploitation. Rural isolation, the supposed cause of bigotry and irrational attachment to outworn custom, was irrevocably ended. The social bonds that delayed the formation of a mobile work force gave way, as peasants were required to supply labor and unlimited resources to the industrial cities with no promise of material gain or social security for some time to come. With very different social goals in view, both prerevolutionary “state capitalists” and Marxists judged peasant institutions obsolete; evidence of peasant allegiance to traditional guarantees of security was ignored or taken as proof of stupidity or blind conservatism. A new, wider, more prosperous world was promised to replace the backwardness, superstition, and, above all, low productivity of a peasant economy.

Among Russian Marxists, such perspectives on the peasant and the village community were axiomatic; even the most rancorous party debates on agrarian policy did not open them to question. But this unbroken consensus, so rare in the history of Russian Marxism, did not necessarily prove that the peasantry’s

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<sup>1</sup> Smith, *The Wealth of Nations*, ed. Max Lerner (New York, 1937), 126–27.



potential either for change or for survival had ever been clearly understood. This essay will trace the early Russian Marxist approach to the problem of rural development in the late nineteenth-century writings of G. V. Plekhanov and V. I. Lenin. It will focus in particular upon the economic evidence, experience, and assumptions that shaped their visions of Russian rural progress.

WHEN EDUCATED RUSSIANS of Plekhanov's time attempted to generalize about rural development, they frequently took their economic evidence from the massive statistical investigations undertaken in the last third of the nineteenth century by provincial *zemstvo* organizations and funded by the tsarist regime.<sup>2</sup> In some quarters, the *zemstvo* data were suspect, tainted by methodological inconsistency and "populistic" bias. There was good reason for such charges; *zemstvo* research was unsophisticated and frequently inconsistent both in data-gathering procedures and in methods of statistical analysis. Although the social origins and political stance of the investigators ranged from V. I. Orlov, a "moderate" *raznochinets*,<sup>3</sup> to S. N. Iuzhakov, a general's son who became an advocate of international peasant revolution, many did indeed share a "populistic" commitment to the immediate improvement of the material life of the masses. *Zemstvo* statisticians took seriously the repartitional land commune (the *mir* or *obshchina*), to which the overwhelming majority of the peasants belonged.<sup>4</sup> In contrast to the Marxists, they insisted that the commune's collectivist practices affected peasant values and behavior and, therefore, had to shape any realistic strategy for rural development.

Nevertheless, whatever their populist hopes may have been, the findings of *zemstvo* statisticians provided little reason for unmixed optimism about any rural institution or social group in nineteenth-century Russia. One of the most in-

<sup>2</sup> I have not attempted an exhaustive survey of the *zemstvo* investigations; my concern has been to establish the complexity of the evidence and the judgments of the researchers whose works were most often cited by Marxists during the last two decades of the nineteenth century. My choice of sources has been influenced by Steven Grant, "The Peasant Commune in Russian Thought, 1861-1905" (Ph.D. dissertation, Harvard University, 1973). For a useful discussion of the methods used by *zemstvo* statisticians, see N. A. Svavitskii, *Zemskie podvornye perepisi* (Moscow, 1961), esp. 129-212.

<sup>3</sup> *Raznochinets* (pl., *raznochintsy*): The *raznochintsy* were individuals of nongentry origin who managed, through education and effort, to escape the calling of their fathers. Career opportunities were not abundant for such people in imperial Russia; the most famous *raznochinets* of the reform era was N. G. Chernyshevskii, a priest's son who went on to become a literary critic, novelist, and revolutionary.

<sup>4</sup> Before and after the emancipation of the serfs in 1861, local, territorially based communes guaranteed each member household's right to farm an allotment comprised of scattered strips of land. Allotments were periodically redistributed among members of the commune according to family size, the number of able-bodied workers per family, or some other collective social principle. Tools and livestock were family property, but the commune's authority was absolute in decisions about the frequency and method of repartition and in all questions relating to the crop cycle. In western and southern Russia, where limited forms of hereditary (*podvornoe*) tenure prevailed, village assemblies (*skhody*) rather than private individuals generally decided when crops were to be sown or harvested. Local disputes were decided by the *skhod* or by elected peasant courts on the basis of customary laws, which treated labor within the family and community as the most powerful claim to ownership, use, and inheritance of property. Although such practices and customs pervaded Russian rural life, it was the commune that came to embody the tradition of "peasant collectivism." For a useful discussion of the often problematic distinction between rights of peasant landownership and use within and outside the commune, see K. R. Kachorovskii, *Russkaia obshchina, vozmozhno li, zhelatel'no li ee sokhranenie i razvitiie?* (*Opyt tsifrogo i fakticheskogo issledovaniia*), 1 (St. Petersburg, 1900): 3-21; Kachorovskii's discussion of peasant law and practice is generally derived from A. I. Efimenko, *Issledovaniia narodnoi zhizni* (Moscow, 1878).

fluent studies of the 1870s was carried out by Orlov and his assistants, who visited fifty-five hundred settlements in Moscow province in an attempt to persuade peasants to respond to a questionnaire about their economic practices. A summary of Orlov's findings published in 1879 reveals economic inequality, market relationships, and abysmally backward agricultural practices both inside and outside the supposedly egalitarian commune.<sup>5</sup> According to Orlov, the very rich and the very poor were deserting the commune, while the kulak moneylenders who remained manipulated the village assembly's decision-making process to exclude the poor and even deprive them of their allotments. In the thirteen noncommunal (*podvornoe*) settlements in Moscow province, conditions were worse: there, divisions between rich and poor were far greater, while the level of agricultural technique was equally low.<sup>6</sup>

The statistical evidence gathered by Orlov's contemporaries provided a complex, even contradictory, picture of the Russian countryside. In Kursk, Tambov, Poltava, and Bessarabia, the economist N. A. Karyshev found the ratio of livestock to land higher for peasant communes, although only the *podvornoe* settlements had improved systems of crop rotation. The statistician L. Kotelianskii found economic inequality extremely widespread in White Russia and the Ukraine, regions where the commune tradition was weak or nonexistent. These areas were not notably more productive than the commune territory of the North, although Ukrainian land was more fertile. In the South of Russia, V. Ye. Postnikov documented the prosperity of "capitalist" private farms on consolidated plots of land (the *otrub* or *khutor*), but, in the Don region, farmers on private land rapidly exhausted the soil in desperate efforts to adjust to short-term fluctuations of the grain market.<sup>7</sup> And in the 1880s, *zemstvo* statisticians documented a rising demand for communal tenure in areas where it had not previously existed. In general, there was no consistent link between the existence of the commune and the retention of backward agricultural practices.

The most comprehensive summary of *zemstvo* findings to appear in the late nineteenth century was the work of A. F. Fortunatov and V. P. Vorontsov.<sup>8</sup>

<sup>5</sup> Orlov, *Formy krest'ianskogo zemlevladieniia v moskovskoi gubernii*, volume 4, part 1 of *Sbornik statisticheskikh svedenii po moskovskoi gubernii* (Moscow, 1879), 307–08.

<sup>6</sup> Orlov, *Formy krest'ianskogo zemlevladieniia*, 295. These were not altogether novel discoveries. Solid evidence of economic inequality within the commune had been available at least a decade earlier in the work of V. V. Bervi (N. Flerovskii); see Bervi (Flerovskii), *Polozhenie rabochego klassa v Rossii: Nabliudeniia i issledovaniia N. Flerovskogo* (St. Petersburg, 1869). But evidence of rural differentiation came to seem more meaningful to Russian radicals in the wake of their failure to incite supposedly "socialistic" peasants to revolution; see pages 738–39, below.

<sup>7</sup> Karyshev, "Podvornoe i obshchinnoe khoziaistvo: Statisticheskie paralleli," *Russkoe bogatstvo*, no. 1 (1894): 45–67; Kotelianskii, "Ocherki podvornoj Rossii," *Otechestvennye zapiski*, no. 9 (1878): 48; Postnikov, *Iuzhno-russkoe krest'ianskoe khoziaistvo* (Moscow, 1891), 205–39; and P. S. Efimenko, "Programma dlia sobiraniia svedeniia obshchinnom zemlevladieni," *Slovo*, no. 6 (1878): Appendix, 1–40.

<sup>8</sup> Members of the gentry class who had trained as physicians, Fortunatov and Vorontsov sought to understand the poverty and inequality they observed within the "egalitarian" peasant community. Both became statistician-economists—Fortunatov, a professor of economics and statistics at the University of Kiev; Vorontsov, a nonacademic scholar whose studies of Russian economic development became classic works of populist economics. *Itogi ekonomicheskogo issledovaniia Rossii po dannym zemskoi statistiki*, their massive compilation of statistical investigations, was remarkably unpolemical in tone and was even criticized by a reviewer in the populist-oriented *Russkoe bogatstvo* for its failure to provide generalizations that would help the reader interpret the voluminous data provided. See the Review of *Itogi ekonomicheskogo issledovaniia Rossii* (1892), in *Russkoe bogatstvo*, no. 9 (1892): 24.

Their compilations suggest that those peasant agricultural practices that seemed the most irrational were the result of a struggle for survival so desperate that only short-term considerations could be taken into account. A case in point was the frequent repartition of commune land, a phenomenon that fragmented already inadequate allotments and made impossible the formulation of rational economic plans for the future. The investigations summarized by Vorontsov indicate that rates of communal repartition had always risen in times of particular material distress. Fire, famine, or cholera led peasant communities to demand repartitions that took into account significant increases or reductions in the size of member families. After the emancipation of 1861, when commune peasants freed from serfdom began to carry the burden of the state's industrialization program, the rates of repartition accelerated rapidly.<sup>9</sup> Elsewhere, peasants subject to similar financial pressures went into debt or sold their allotments to their wealthier neighbors. The tendency of rich peasants to become moneylenders rather than entrepreneurs suggested to Vorontsov that their prosperity was not even a technically progressive phenomenon.<sup>10</sup>

Despite his sympathy for communal efforts, Vorontsov found that the chief innovators in Russian agriculture were gifted and intelligent individuals acting on their own. Within the commune, a majority had to be convinced before any decisive action could be taken; the speed of change was limited by the experience and intelligence of its members. At the same time, Vorontsov's evidence indicated that, once a majority decision was reached, improved techniques seemed to spread more rapidly and widely in commune areas than on land in private tenure. The use of improved methods and means of cultivation did not appear to be a monopoly of the rich, at least with respect to the use of fertilizer and improved tools and the decision to engage in intensive cultivation. In the black earth zone, where exhaustion of the soil was a relatively recent phenomenon, the gentry used fertilizer as rarely as the peasants did either inside or outside of the commune. In other parts of the empire, with the exception of the Baltic provinces, peasants in commune territory used fertilizer more intensively than *podvornoe* peasants or the gentry. Where improved tools were cheap and immediately useful, Vorontsov discovered no "natural" peasant resistance to innovation; peasant demand for a form of improved plow (*plug*), for example, far exceeded the supply.<sup>11</sup> Given the generally high price of farm implements (in the 1880s, many imported tools were taxed in order to protect Russian infant industry), the real question seemed to be whether the peasant paid out, in ex-

<sup>9</sup> The conditions of emancipation imposed by the tsarist regime left most peasants in communes but with diminished allotments of land, for which the peasants were required to pay a price fixed far above its market value. Subsequently, the peasantry's tax burden began to escalate, as the sale of Russian grain abroad became a weapon in the government's struggle to accumulate capital, subsidize Russian industry, and maintain Russia's position as a great power. See V. I. Skalon, "Krest'ianskii bank i ego nedomshchiki," in A. A. Manuilov, ed., *Ocherki po krest'ianskomu voprosu*, 2 (Moscow, 1905): 1-71.

<sup>10</sup> Vorontsov, *Krest'ianskaia obshchina*, volume 1 of *Itogi ekonomicheskogo issledovaniia Rossii po dannym zemskoi statistiki*, ed. Fortunatov (Moscow, 1892), 127-28, and *Progressivnye techenii v krest'ianskom khoziaistve* (St. Petersburg, 1892), 147-51.

<sup>11</sup> Vorontsov, *Progressivnye techenii v krest'ianskom khoziaistve*, 147-51, 218-61.

change for the plow, a share of the crop that was larger than the difference that such a plow might make in the productivity of the family allotment.

The statistical evidence gathered by the *zemstvo* investigators was as difficult to summarize as it was to interpret. For those who sought a coherent analysis of rural differences, *otrub* productivity, kulak conservatism, and commune efforts at modernization, the temptation to rely upon Western experience to explain and simplify Russian developments was powerful indeed. For our purposes, it was particularly important that N. I. Ziber and A. I. Skvortsov, the professional economists whose work was cited as authoritative by Russian Marxists of the nineteenth and early twentieth centuries, engaged in no independent research on Russian rural life, relied heavily upon secondary literature to support their conclusions, and considered *zemstvo* statistics an indisputable proof that capitalism represented progress in the agricultural economy of the 1880s.

Ziber, a liberal-minded professor at the University of Kiev, was a fanatic proponent of Marxism as the scientific culmination of classical economic theory.<sup>12</sup> According to Ziber, Marx had demonstrated beyond a doubt that the laws of capitalist development were absolutely invincible and purely economic in character. Committed to a thoroughly mechanistic economic determinism, Ziber argued that capitalism emerged "naturally" out of more primitive, communal property formations. Once the commune reached a certain level of productivity, its property structure became a "fetter," an obstacle to further development, so that capitalism was the only appropriate mechanism for the efficient organization of labor and productive investment of surplus.<sup>13</sup> Ziber claimed that in Russia the commune was rapidly disintegrating because of the inevitable conflict between rural proletarians and peasant capitalists that developed in the course of the commune's internal decline. Although he did not support his theory of increasing rural stratification with a mass of empirical evidence, Ziber remained confident that the "universal" laws of economic science would triumph over traditional commune limits on rural differentiation. As he remarked to the populist N. K. Mikhailovskii, Russian progress required that the peasant be "cooked up in the industrial boiler" and presumably emerge a proletarian.<sup>14</sup>

A. I. Skvortsov, who taught agricultural economics at the Novaia Alexandriia University in Russian Poland, was more politically conservative and less schematic than Ziber. Skvortsov combined belief in Marxist economic analysis with support for the Orthodox Church, constitutional monarchy, and Great Russian nationalism. His primary research dealt with the relationship between railway building and nineteenth-century English economic growth. When he wrote about rural development, he praised industrious English tenants and landlords who made economically productive decisions about land use. In his view, the English were particularly fortunate in their freedom from "medieval" social commitments. They could move in search of better work situations, negotiate

<sup>12</sup> *Das Kapital* was "but a continuation and a development of the same principles upon which the doctrine of Smith and Ricardo has been built"; Ziber, *Izbrannye ekonomicheskie proizvedeniia* (1881; Moscow, 1959), 556.

<sup>13</sup> Ziber, *Izbrannye ekonomicheskie proizvedeniia*, 683.

<sup>14</sup> Ziber, as quoted in Mikhailovskii, *Polnoe sobranie sochinenii*, 9 (St. Petersburg, 1903): 327-28.

binding but limited contracts with landlords and employers, and take full responsibility for financial success or failure. But Russian peasants, who had as great a potential as English farmers for competitive, profit-oriented activity, were trapped by a system of land repartition that permitted the envious and lazy to acquire allotments improved by the labor or capital of the more ambitious and industrious.<sup>15</sup> Less consistent than Ziber, Skvortsov gloried in the virtues of the free market, while hoping that decisive action by the state would nevertheless ensure the salvation of both the peasantry and the commune. Skvortsov proposed that the head of each peasant household be granted the hereditary right to use a single, consolidated plot of land without restriction; at the same time, he wanted the state to protect less competent peasants by preserving communal tenure and prohibiting the sale or lease of their allotments. Skvortsov hoped that such a combination of freedom and security would liberate the energies of the enterprising peasant and protect the rest from proletarianization. Convinced that firm and benevolent government action might succeed in raising Russian agricultural productivity to the level of England's, he feared that inaction would doom both the peasantry and the commune.<sup>16</sup>

Most leading professional economists of the day looked at far more *zemstvo* evidence than did Ziber and Skvortsov and arrived at very different conclusions as to its meaning and implications. Impressed by the emergence of kulaks and proletarians and by the high productivity of the *otrub* and the *khutor*, A. I. Chuprova and A. S. Posnikov were troubled as well by the at least short-term economic and social disaster that the independent farmer often brought to his neighbors. Many observers recognized that in the South and West farmers on consolidated private farms were eliminating the practice of compulsory cultivation.<sup>17</sup> A paragon of economic virtues in some respects, the *otrubnik* was not infrequently a moneylender, who purchased the land of his delinquent neighbors and then hired them to work for a pittance on the allotments they had lost. In this context, "populistic" economists like Fortunatov were hard put to reconcile the apparent conflict between productivity and immediate peasant welfare. They were equally unwilling to rely upon the state's industrialization program to absorb Russia's increasing labor surplus, because new industrial growth was destroying the peasantry's part-time artisanal occupations more rapidly than Russian infant industry could provide alternative sources of income.<sup>18</sup> With varying degrees of optimism, these economists chose among what they considered to be more or less unsatisfactory alternatives and recognized the merit of a relatively slow (and, for Vorontsov, all the more profound) trend toward rural innovation within the commune. "Populistic" writers attached particular signif-

<sup>15</sup> Skvortsov, *Ekonomicheskie prichiny golodovok v Rossii i mery k ikh ustraneniui*, volume 1 of *Ekonomicheskie etiuudy* (St. Petersburg, 1894), 84–85. Skvortsov's discussion of the relationship between repartition and economic incentive was based primarily upon the research of the German economist Johannes V. Keussler. Both Keussler and Skvortsov attempted to prove that communal practices were incompatible with economic progress by making use of the work of investigators who found the commune a viable institution. In particular, see Keussler's use of L. V. Khodskii's work in Keussler, *Zur Geschichte und Kritik des bauerlichen Gemeindebesitzes in Russland*, 3 (St. Petersburg, 1887): 82–87.

<sup>16</sup> Skvortsov, *Ekonomicheskie prichiny golodovok v Rossii*, 152–53.

<sup>17</sup> See, for example, Posnikov, *Iuzhno-russkoe krest'ianskoe khoziaistvo*, 205–39.

<sup>18</sup> A. F. Fortunatov, "Raspredelenie pozemel'noi sobstvennosti v evropeiskoi Rossii," *Russkaia mysl'*, no. 7 (1887): 33–47.



icance to the evidence that commune peasants in Novgorod, Moscow, and Tver provinces had successfully inaugurated intensive farming on their own initiative in the 1880s.<sup>19</sup>

The economists A. I. Chuprov and A. S. Posnikov also found Western models relevant to Russian rural development, although their version of the West bore little resemblance to the Anglophile perspectives of Ziber and Skvortsov. Posnikov studied the relationship between forms of tenure and the level of rural development in England and Germany and discovered that tenants and governments, rather than private landowners, were the chief agents of economic innovation. Noting as well that English tenant contracts were comparable in length to the intervals between communal repartition, Posnikov concluded that security of tenure was not a prerequisite for rural innovation.<sup>20</sup> Chuprov's economic analysis of Danish agriculture suggested that the small producer was frequently more able to adjust to fluctuations in the international market than the large-scale entrepreneur.<sup>21</sup> "Westernizers" all, in search of lessons and examples that promised a decent future for the majority of the Russian peasants, neither Chuprov nor Posnikov nor their "populistic" contemporaries believed that the small producer was doomed because English agricultural achievement had coincided with the proletarianization of the peasantry.

FOR RUSSIAN RADICALS, it was significant that such a massive and complex body of evidence and interpretation appeared at a time of unprecedented Western power and prestige, when their own efforts to spark a peasant revolution had failed. In the course of the nineteenth century, the increasingly spectacular manifestations of Western military might and economic success in China, India, and Africa dazzled and terrified educated Russians, who feared—with some reason—that Russia might become an India if it did not somehow become an England. They were equally impressed with a variety of tough-minded theories that seemed to explain Western achievement. The iron laws of Ricardo, Malthusian dangers of overpopulation checked only by "famine and vice," and the Darwinian "struggle for existence" were taken by many as proof that human progress depended upon the political and economic stimulus provided by insecurity and conflict. In the Promethean upward struggle by classes (Marx) or by nations (Friedrich List), peasants and peasant societies seemed inevitably doomed. As Marx had written in *Das Kapital*, "The system of small proprietorship must be annihilated: it is annihilated. Its annihilation, the transformation of the pygmy property of the many to the huge property of the few . . . , this fearful and painful expropriation of the masses of the people forms the prelude to the history of Capital."<sup>22</sup> The new, "universal" laws of science and scientific history contained within them a vision of progress that set no limits on the amount of human suf-

<sup>19</sup> See, for example, L. S. Lichkov (L. I. Ch-kov), "Intensivnaia kul'tura i obshchinnoe zemlevladienie," *Russkaia mysl'*, no. 6 (1887): 33–47.

<sup>20</sup> According to Posnikov, if tenancy did not check economic incentive within the English economy, then insecurity of tenure due to communal repartition could not be considered an insuperable obstacle to Russian rural innovation; Posnikov, *Obshchinnoe zemlevladienie*, 1 (Odessa, 1875): 7–55, 73–80.

<sup>21</sup> Chuprov, *Krest'ianskii vopros* (Moscow, 1909), 80–87.

<sup>22</sup> Marx, *Capital*, trans. Samuel Moore and Edward Aveling, 1 (1869; London, 1909): 835.

fering required before economic prosperity could be attained. The future was particularly bleak for social groups or nations that did not forcefully and effectively enter into the capitalist struggle for existence.

In the mid-nineteenth century, the Russian revolutionary movement was rooted in a denial that the laws of Malthus, Ricardo, or even Darwin would dictate Russia's future development. "Repentant nobles" like Alexander Herzen had found no redeeming virtues either in the history of French middle-class struggles for the rights of private property or in the horrors of primitive accumulation manifest in the grey and gloomy streets of Leeds and Manchester. After Herzen, populists like N. G. Chernyshevskii, P. L. Lavrov, and N. K. Mikhailovskii argued (some more ambivalently than others) that peasant rebellion combined with the cooperative and democratic practices of the commune might provide the starting point for socialism and a more humane form of economic progress than the world had yet witnessed.<sup>23</sup>

In the 1870s, thousands of radical students attempted to realize such hopes in a "movement to the people" aimed at revolutionizing Russian rural life. Peasant responses to the student "incursion" were inconsistent. Some were suspicious and hostile, while others cautiously welcomed the aid and counsel provided by Vera and Lydia Figner, Peter Kropotkin, and Catherine Breshko-Breshkovskaia. But the peasants, whatever they may have felt about these idealistic young people, did not launch a socialist revolution. (The only documented case of rebellion took place in the Chigirin district, where radicals distributed forged manifestoes from the tsar urging peasants to kill the landed gentry.<sup>24</sup>) As a consequence, some of the most zealous student radicals, frustrated in their quest for immediate and enthusiastic acceptance, began to suspect that peasants were hopelessly backward, incapable of constructive economic or political action.

At best, such judgments were premature. In the 1870s, as G. V. Plekhanov, Vera Zasulich, and Lev Deutsch struggled against their bitter disappointment at peasant apathy, the "backward" peasants over-enrolled in *zemstvo* schools, voting a healthy percentage of commune funds for educational purposes.<sup>25</sup> In the areas where radical students had been active, field workers went on strike for higher wages in the 1880s, and peasant brotherhoods sprang up in the last decade of the nineteenth century.<sup>26</sup> The late-nineteenth-century shift of radical ideological loyalties from a "stagnant" peasant majority to a "dynamic" proletarian minority<sup>27</sup> was thus not simply the product of a more realistic appraisal of the Russian situation. Equally important as contributing factors were a will-

<sup>23</sup> For good general discussions of the origins of the Russian revolutionary movement, see Franco Venturi, *Roots of Revolution* (New York, 1960), 1-203; James Billington, *Mikhailovskii and Russian Populism* (New York, 1958); and William Woehrlin, *Chernyshevskii: The Man and the Journalist* (Cambridge, Mass., 1971).

<sup>24</sup> See Daniel Field, *Rebels in the Name of the Tsar* (Boston, 1976).

<sup>25</sup> Patrick Alston, *Education and the State in Tsarist Russia* (Stanford, 1969), 59-66.

<sup>26</sup> Maureen Perrie, *The Agrarian Policy of the Russian Socialist-Revolutionary Party from Its Origins through the Revolution of 1905-1907* (Cambridge, 1976), 14-23.

<sup>27</sup> According to official accounts there were 224 factory strikes between 1870 and 1879; see A. M. Pankratova, Introduction: "Osobennosti formirovaniia i bor'by proletariata Rossii v 60-80-x godakh XIX veka," in Pankratova, ed., *Rabochee dvizhenie v Rossii v XIX veke: Sbornik dokumentov i materialov*, 2, pt. 1 (Moscow, 1950): 45.

ingness to attribute universal and scientific significance to their political frustrations, an uncritical view of Western culture, and a very careful selection of evidence from the *zemstvo* statistics.<sup>28</sup>

A number of the disillusioned populists of the 1880s used England as their Marxist model for Russia's rural development. Since the English agricultural economy was both "peasantless" and in a recession at the time, their choice was by no means self-evident. But progressive intellectuals in Russia and elsewhere found it logical to assume, as Marx himself had done, that England provided, for the less developed nations of the world, "the image of their future."<sup>29</sup> They were particularly vulnerable to the suggestion that the power and culture of the city would inevitably destroy the economic and social life of the peasant majority in the world's population. In the wake of peasant ruin, the more dynamic, rational, and, above all, urban social classes (to which the radicals themselves belonged) would go on to create the material basis for a humane and just society.

In the Marxist classics, they found a vision of progress that was clear and relatively simple, with no place for the small producer. In the "Communist Manifesto" and *Das Kapital*, the bourgeoisie and the proletariat appeared as the only modern agents of revolutionary social transformation; the ubiquitous peasantry was a transitory group whose virtues were exaggerated by sentimental idealists. Although Marx was neither precise nor consistent in his use of the term, it would be fair to say that, in general, he considered the peasant a remnant of feudal society or an incipient capitalist or proletarian—either an ineffective obstacle to modern capitalist development or a member of the petty bourgeoisie, whose sympathies lay with the exploiters and whose fate, in most cases, was to be oppressed.<sup>30</sup>

Marx rarely discussed peasant institutions, and the observations he made on the subject of rural communities were neither profound nor grounded in the scholarship that supported his political and economic generalizations. In his major works, petty bourgeois peasants had no more in common than "potatoes in a sack," while peasant communes were archaic survivals from the stage of "primitive communism."<sup>31</sup> The meagerness of his comments was consistent with a general belief (not at all uncommon in late-nineteenth-century Europe) that peasants and peasant institutions were obsolete from a historical point of view. On a deeper level, however, the very concept of community was fundamentally at odds with the use of class struggle as a tool of analysis. Because he was as impressed by conflict models of behavior as many of the economists he was at-

<sup>28</sup> For some radical intellectuals, this was only the first stage in a continuing flight from the primitive. In time, E. A. Kuskova and K. D. Prokopovich, for example, found the proletariat too backward for their purposes and moved on to the more rational, progressive liberals of the gentry and middle classes as the most appropriate focus of their hopes for change.

<sup>29</sup> For a discussion of the English agrarian crisis of the late nineteenth century, see William Ashworth, *An Economic History of England* (London, 1960), 53–77.

<sup>30</sup> For a particularly illuminating discussion of Marx's use of the term "peasant," see V. M. Chernov, *K teorii klassovoi bor'by* (Moscow, 1906), 19–20.

<sup>31</sup> Marx's *Grundrisse* manuscripts provide perhaps the best insight into his approach to the problem of community. See Marx, *Pre-Capitalist Economic Formations*, ed. Eric Hobsbawm (New York, 1964), 79–83.

tempting to supersede were, Marx found the struggle between backward peasants and a modernizing gentry or between rural entrepreneurs and farm hands infinitely more worthy of study than the "illusory" bonds that united the peasants in their territorially based communities. Whatever the objective merits of such judgments, they clearly posed serious problems for revolutionaries outside Western Europe. The Marxian approach seemed to place Marxists in a peasant society in a potentially hostile relationship to the majority of the people in the societies they intended to liberate.

In fact, both Marx and his first Russian "students" were equally aware that his work might have limited application to Russia. Marx found it "ironic" that the Russian populist N. F. Daniel'son, rather than a French or English socialist, should be the first translator of *Das Kapital*, while Marx's early Russian sympathizers were for their part extremely selective in their praise.<sup>32</sup> The social critic Mikhailovskii, suspicious of the uses to which a Russian Marxism might be put, wanted to know if Marxian theory prescribed capitalism as an inevitable stage of every nation's historical development. Even Daniel'son, who considered himself a Marxist, found it necessary to demonstrate that a Marxist analysis could indeed be relevant to the Russian situation. In his massive investigation of the social relations of Russian agricultural production between 1861 and the late 1880s, Daniel'son found the capitalist practices that were emerging in the Russian city and countryside fundamentally incompatible with the immediate needs of the majority of the population. He therefore argued that Russia had no choice but to

lean on our historical inheritance and cease to destroy our ancient historical form of production, a form based upon direct producers. This is necessary in order to avoid the danger that threatens every nation that departs from the age-old foundations of its welfare. All efforts must be directed toward a unification of agriculture and manufacturing industry in the hands of the direct producers, but a unification not on the grounds of a small-scale fragmented production unit, which would be tantamount to the "decree of universal mediocrity," but on the grounds of the creation of a massive socialized production based on the free development of science and technology, with the aim of satisfying the genuine requirements and well-being of the whole population.<sup>33</sup>

Marx's response to populist challenges was impressively free of dogmatism or wounded pride. Instead of referring his critics to the appropriate page of *Das Kapital*, he assured Mikhailovskii that his studies of Western Europe were not intended to be a "historico-philosophical" theory of development to be imposed on all peoples regardless of their concrete historical circumstances.<sup>34</sup> Encouraged by Daniel'son, Marx learned Russian well enough to read *zemstvo* statistical

<sup>32</sup> Marx, *Letters to Dr. Kugelmann* (New York, 1934), 77. Both Vorontsov and Mikhailovskii cited passages from *Das Kapital* in order to counter the arguments of Russian industrialists for the progressive economic benefits of child labor. See, for example, N. K. Mikhailovskii, *Sochineniia*, 6 vols. (St. Petersburg, 1896-97), 1: 169-74.

<sup>33</sup> Daniel'son, *Ocherki nashego poreformennogo khoziaistva* (St. Petersburg, 1893), 344.

<sup>34</sup> Marx, "Letter to Mikhailovskii" (1877), in Dona Tor, trans., *Karl Marx and Friedrich Engels: Selected Correspondence, 1846-1895* (New York, 1942), 353-54. Marx's letter to Mikhailovskii was never sent. After his death, it was translated into Russian, where it appeared first in the organ of the People's Will, *Vestnik narodnoi voli* (1886). See the discussion in Richard Pipes, *Struve: Liberal on the Left, 1870-1905* (Cambridge, Mass., 1970), 47.

analyses on his own. And, in his first attempt to examine the problems raised by a contemporary conflict between capitalist and noncapitalist social formations, Marx came down on the side of Daniel'son. By 1881, Marx had concluded that "the special research into this subject that I conducted, the materials for which I obtained from original sources, has convinced me that this community is the mainspring of Russia's social generation, but, in order that it might function as such, one would first have to eliminate the deleterious influences that assail it from every quarter and then to ensure the conditions for spontaneous development."<sup>35</sup>

It is understandable that these guardedly "populistic" statements, expressed in a series of letters written shortly before his death, were not taken by Marx's foremost admirers as a substitute for the argument presented in his major works. Whether aspiring Russian Marxists of the 1880s and 1890s looked to *The Eighteenth Brumaire of Louis Bonaparte* or to *Das Kapital*, they learned that the "universal mediocrity" created by a system of small property relations deprived the peasant of either the will or the intelligence to build a modern and productive agriculture, much less a successful revolutionary movement against capitalism. And it was not Daniel'son who became known as the father of Russian Marxism but G. V. Plekhanov, a very different sort of thinker.

A FRUSTRATED VETERAN of the "movement to the people," Plekhanov found in the evidence of kulak greed and competition provided by the *zemstvo* statistics a kind of absolution for the populist failure to spark a popular revolution and a justification for the most thoroughgoing rejection of the peasantry. Presumably, if class struggle within the commune had already destroyed the basis for socialist action by the peasantry as a whole, the failure of radicals to spark a peasant revolution could not be ascribed to their political incompetence. A reading of Orlov's statistical summary convinced Plekhanov that capitalism was in the process of "dislodging the independent producers from their unstable conditions and creating an army of workers in Russia by the same tried and true methods that had created modern English society."<sup>36</sup> Plekhanov saw no reason to idealize a peasantry he found hopelessly devoid of "solidarity, broad social interests, or ideas."<sup>37</sup> Instead, he looked to the revolutionary Russian bourgeoisie, which was

<sup>35</sup> K. Marks and F. Engels, *Sochineniia*, 19 (2d ed., Moscow, 1961): 250–51. Marx worked very carefully on this formulation. For earlier drafts of this passage, see *ibid.*, 400–21. For a brilliant discussion of the relationship between Marx and his first Russian disciples, see Andrzej Walicki, *The Controversy over Capitalism: Studies in the Social Philosophy of the Russian Populists* (Oxford, 1969), 179–94.

<sup>36</sup> Plekhanov, *Sochineniia*, 24 vols. (Moscow, 1923), 2: 225. Nevertheless, until at least 1880, Plekhanov remained an ambivalent populist. He had come to believe that industrial workers would provide the "chief forces" for a revolutionary socialist movement, because their rural ties sustained an interest in the land question at the same time that their urban surroundings provided them an escape from the idiocy of rural life. Plekhanov justified his continuing concern with the peasantry on the basis of the still insignificant number of urban workers and the relatively low level of Russian industrial development. Although these reasons remained valid for at least two more decades, they did not prevent Plekhanov from making his way toward an exclusively proletarian-centered Marxism. See L. G. Deutsch, "Kak G. V. Plekhanov stal marksistom," *Proletarskaia revoliutsiia*, 8 (1933): 118.

<sup>37</sup> Plekhanov, *Sochineniia*, 2: 221–23.



to come forward to champion the "Rights of Man" just as the French had done in 1789.<sup>38</sup> Giving vent to his bitterness and disillusionment (but always in the guise of scientific social analysis), he judged the Russian peasantry no better than the Chinese, "barbarian tillers of the soil, cruel and merciless, beasts of burden whose life provided no opportunity for the luxury of thought."<sup>39</sup>

Since Plekhanov had read little Russian economics before 1885, it may be unjust to attempt to derive his perspectives on rural development from *Our Differences*, a polemical attack on the populists published in that year. In the 1890s, Engels himself suggested that the time had come for Plekhanov to engage in "serious" economic research instead of polemics on the agrarian question.<sup>40</sup> But in *Our Differences* and in later books and articles, Plekhanov preferred to attack the motives, intelligence, and honesty of Mikhailovskii, Daniel'son, and Vorontsov rather than to confront the complex evidence contained in the varied and extensive studies of contemporary statisticians and economists. On the basis of his meager research, Plekhanov began in the 1880s to construct the immensely influential myth that "subjective" populists ignored economic evidence, while Marxists were rigorous economists, solidly in touch with the facts. Although Plekhanov did not make the foolish claim that either kulaks or proletarians were numerically predominant, the populist focus upon the overwhelmingly peasant majority of the population became, nevertheless, his proof of their sentimentality. By Plekhanov's standards, only those writers who agreed that the differentiation of the peasantry into antagonistic social classes was the central fact of Russian rural life qualified as realists in the field of economic analysis.

In *Our Differences*, Plekhanov selected evidence from several *zemstvo* investigations to demonstrate a truth that was already widely accepted among economists (though relatively new to the general reading public). He pointed out that, within the "egalitarian" commune, wealthier peasants purchased livestock and agricultural machinery, rented equipment out to the poorer, and thus drove them further into debt as the level of productivity rose. Plekhanov was certain that the kulaks, once launched on such ventures, would inevitably replace the old-fashioned gentry as the proprietors of large-scale economic enterprises (thus vindicating the Marxian law of concentration at a time when gentry estates were in decline). Kulak wealth and power made a mockery of the populist belief that all commune peasants were somehow equally capable of working their fields on an improved system.<sup>41</sup> Plekhanov claimed that, in fact, peasants

<sup>38</sup> Although Plekhanov did not claim that kulaks were a politically progressive force, he insisted that "anyone who has paid the least attention to the course of Russian life in the past decade knows that the interests of the Russian bourgeoisie have now come to implacably contradict the interests of absolutism"; *Sochineniia*, 2: 203.

<sup>39</sup> Plekhanov, *Sochineniia*, 10: 129.

<sup>40</sup> *Reminiscences of Marx and Engels* (New York, n.d.), 328-33.

<sup>41</sup> Plekhanov, *Sochineniia*, 2: 255-60. According to Plekhanov, even a program of government tax relief would only enable the rich to fight with increased effectiveness to gain control of the allotments of the poor. He found suggestions for land reform ludicrous, given the political realities of the time: "Let us suppose that the peasant commune is really our anchor of salvation. But who will carry out the reforms postulated by Nikolai — On? [shorthand for "Daniel'son"] The tsarist government? Pestilence is better than such reformers and their reforms! Socialism being introduced by Russian policemen—what a chimera . . ."; Plekhanov, *Peripiska K. Marksa i F. Engel'sa s russkimi politicheskimi deiateliami* (1931), 334, as quoted in Walicki, *The Controversy over Capitalism*, 127.

were better off as free tenants or hired hands, at least until the proletariat was able to carry out a socialist revolution for the liberation of peasants as well as proletarians. Dynamic capitalist relations would at least destroy the weakness and inertia that, as he put it, "give such pain to educated people in any backward agricultural society."<sup>42</sup>

The harshness of Plekhanov's statements came back to haunt him during the famine year of 1891.<sup>43</sup> For populists like Mikhailovskii, famine was the result of a state-sponsored modernization program that compelled peasants to eat less in order to sell grain to pay increased taxes. Mikhailovskii bitterly suggested that Plekhanov and his fellow Marxists should welcome the famine; if more urban proletarians were needed in order to achieve a socialist revolution, then any disaster that succeeded in driving the small producer from the land was clearly a positive step forward.<sup>44</sup> Plekhanov's response to Mikhailovskii's challenge was somewhat contradictory. On the one hand, he claimed that the process of proletarianization did not depend upon Marxist "welcome" or populist "hostility"; it was futile, he said, to reject the reality of capitalism for the sake of sentimental regrets for the peasants' plight.<sup>45</sup> The famine was, for Plekhanov, an "Asiatic catastrophe," a product of unprecedented population increase and the low productivity associated with the survival of precapitalist rural relationships. On the other, he argued that "the better the conditions of the peasantry, the sooner the commune would break up. That breakup, moreover, can take place in conditions that are either more or less advantageous for the people. The 'pupils' of Marx must 'strive' to see to it that the breakup takes place in the conditions most advantageous for the people."<sup>46</sup> Since he never spelled out the precise nature of the "advantageous" conditions and avoided this issue in the future, Plekhanov presumably had no further answer to Mikhailovskii's challenge.

In the 1890s, when Plekhanov began to sample *zemstvo* statistics more widely, his certainty that peasants were hopelessly irrational makes his analysis of evidence seem curiously naive and literal-minded. In his extended polemic "Against Populism" (1898), he noted repeatedly, with expressions of pained astonishment, that peasants were simply incapable of understanding that alternatives existed to their life in the commune. In reviewing accounts that document peasant loyalty to the commune, he wondered at the barbaric primitive's striking inability to grasp the advantages of private ownership, even when explanations were repeated "ten times over in ten different ways."<sup>47</sup> Easily con-

<sup>42</sup> Plekhanov, *Sochineniia*, 2: 271.

<sup>43</sup> Both the extent and the causes of the famine are still under debate. The evidence and the implications of the famine remain as problematic and difficult to interpret today as they were in the nineteenth century. See Richard Robbins, *Famine in Russia, 1891-2: The Imperial Government Responds to a Crisis* (New York, 1975); James Simms, Jr. "The Crisis in Russian Agriculture at the End of the Nineteenth Century: A Different View," *Slavic Review*, 36 (1977): 377-98; and G. M. Hamburg, "The Crisis in Russian Agriculture: A Comment," *ibid.*, 37 (1978): 481-86.

<sup>44</sup> N. K. Mikhailovskii, *Literaturnye vospominaniia i sovremennaia smuta*, 2 (St. Petersburg, 1900): 270-73.

<sup>45</sup> Plekhanov, *Sochineniia*, 7: 279. The legal Marxist P. B. Struve was far less ambivalent than Plekhanov on this issue. As Struve wrote in 1892, "of the economic independence of the Russian peasant (even previously highly questionable) soon not even a trace will remain. And the famine will deserve much credit for this service"; Struve to A. N. Potresov, as quoted in Pipes, *Struve: Liberal on the Left, 1870-1905*, 86.

<sup>46</sup> Plekhanov, *Sochineniia*, 3: 419.

<sup>47</sup> *Ibid.*, 9: 132-33.

vinced by any explanation that denied the possibility of conscious action by an untutored peasantry, it never occurred to Plekhanov that a peasant was capable of deliberately playing the fool before a master or a *raznochinets* whose motives were considered suspect. And, although Plekhanov himself believed that most peasants would never prosper as rural entrepreneurs, he never imagined that they might, on quite rational grounds, distrust outsiders who preached the merits of independent economic activity. The world of the peasantry presented a uniformly repulsive spectacle to an ex-populist. Peasants fixed in medieval isolation seemed incapable of understanding their own interests; jarred loose by developing capitalism, they pursued their greedy and short-sighted ambitions.

Plekhanov was particularly skeptical about any suggestion that peasants ever acted to protect the welfare of the fellow members of their commune. The work of the statistician F. A. Shcherbina seemed to him especially misleading in this connection. Shcherbina had studied peasant decisions for repartition in Voronezh province, where he found that a concern with what local peasants called "justice" resulted in land divisions that were economically disadvantageous to two-thirds of the commune households in the districts studied. Justice seemed in these cases to relate to such issues as the security of the commune as a whole or the obligation to respond to the demands for repartition that came from young couples without their own household allotments.<sup>48</sup> Plekhanov insisted that the decisions recorded by Shcherbina could have nothing to do with justice, morality, or rational choice. As proof, he cited data from other regions of Russia that suggested repartitions could be a mere formality, which left richer families as powerful as before. He noted as well that, since peasants were collectively responsible for tax payments, the rich in some cases could call for a repartition in order to provide the poor with allotments adequate to meet their tax obligations. As profit-seeking became Plekhanov's Marxist standard for rational economic behavior, any repartitions that were not reducible to the struggle for private gain could only be understood as proof of the peasantry's capacity for ignorant faith in the very institutions that damaged its interests. The widespread demand for the introduction of communal tenure documented by *zemstvo* statisticians in the 1880s suggested to Plekhanov that the peasantry was trapped in an economic situation that could only end with the "complete destruction of the peasants' world view."<sup>49</sup>

Plekhanov's hopes for Russian economic development rested with the potential entrepreneur. In agriculture, this meant the individualist who dreamed of private profit, hired labor, and refrained from improving his allotment for fear that he might lose his investment in any new communal repartition. Plekhanov attached little significance to the actual material situation of the majority of the peasants who, whatever their impulses may have been, neither hired labor nor hired themselves out to work for others. Discarding Skvortsov's benevolent proposals for the salvation of the peasantry and the commune, he used Skvortsov's

<sup>48</sup> Shcherbina, *Sbornik statisticheskikh svedeniakh po Voronezhskoi gubernii* (Voronezh, 1884), 72-73, 86-87.

<sup>49</sup> Plekhanov, *Sochineniia*, 9: 157, 130-63.

work to buttress his arguments for an English-style free-market system, in which tenants negotiated their rents with the landlord and, like their laborers, went elsewhere if a better economic opportunity presented itself.<sup>50</sup> For Plekhanov, rural progress did not begin in the mind of one of Vorontsov's "mythical" average peasants who convinced his fellow members of the commune that innovation was needed; agricultural improvement depended upon the private initiatives of the rising village bourgeoisie. While Vorontsov had suggested that cheap plows were being purchased by millions of peasant households, Plekhanov quoted from the statistical investigations of L. S. Lichkov and V. E. Postnikov, who documented the purchase of modern machinery and thousands of desiatinas of land by kulaks. Plekhanov did not demonstrate that rich peasants were buying all of the plows—and, it should be noted, Vorontsov did not prove that the plows were being purchased by the poor—but Plekhanov's identification of the rich with the economically rational permitted him to argue that populist support for the "progressive" peasant was, in fact, support of the kulaks, the rapacious petty bourgeoisie.<sup>51</sup>

WHEN HIS FIRST PUBLISHED WORK APPEARED in 1893, Lenin was already an enthusiastic and optimistic young Marxist, as certain as Plekhanov was that rural differentiation and the division of labor were creating a work force infinitely superior to the primitive commune peasantry in consciousness and capacity for revolutionary action. In a series of articles and reviews written in the 1890s, he set out to eliminate the confusion that had resulted from what he called "pseudo-scholarly" populist investigations.<sup>52</sup> More politically astute from the outset than Plekhanov, he did not refer to the peasant majority of the population as "barbaric" or "mindless." But Lenin was as hostile as Plekhanov was to the notion of community. Avoiding any extended discussion of the *mir*, Lenin found the Russian peasantry lacking in social solidarity and historically significant institutions, caught between its former feudal masters and a greedy bourgeoisie. Certainly, any Russian of the 1890s who read only Lenin's writings could not have guessed that the repartitional land commune still commanded the loyalty of the overwhelming majority of the population. His omissions made the populist claims for the commune appear particularly foolish—pure assertions rather than judgments based on more statistical evidence than Lenin himself ever confronted.

Since most populist and nonpopulist investigators agreed that the commune failed to prevent rich peasants from lending money at usurious interest rates to the needy, it was easy for Lenin to show that the commune functioned to immobilize the weakest members of rural society within a wretched economic situa-

<sup>50</sup> As a result, English tenants, proprietors, and hired laborers were in Plekhanov's view more likely than commune peasants to make rational economic decisions. And, although he believed that the large enclosed private farm was the more efficient rural production unit, even *podvornoe* tenure seemed to him a progressive step beyond the "economic stagnation" of the commune. Plekhanov, *Sochineniia*, 9: 161, 180, 190.

<sup>51</sup> Plekhanov, *Sochineniia*, 9: 183–84, 2: 257–60.

<sup>52</sup> Lenin, *Polnoe sobranie sochinenii*, 55 vols. (5th ed., Moscow, 1958–65), 1: 276, 285.

tion that forced them to enter into semifeudal relationships with the gentry. By failing to discuss the evidence that had led Chuprov, Posnikov, Karyshev, Shcherbina, Lichkov, and Postnikov to find the commune a viable institution, he could portray the populist emphasis upon the constructive aspects of the commune as a kind of reactionary "economic romanticism."<sup>53</sup> In 1893, Lenin denounced the economist Karyshev as a "liberal cretin" for suggesting that the commune contained equalizing mechanisms that might diffuse the benefits of agricultural improvement among its poorer members. As he characteristically put it, "Is this naiveté or is it something worse?" But, in general, Lenin seldom referred directly to the work of professional economists. Instead, he focused on the deficiencies of Vorontsov, Daniel'son, and Mikhailovskii, discussed the segments of various statistical investigations that supported his view of rural differentiation, and cited authoritative statements from Skvortsov. Concerned above all with the material conditions that created the competitive entrepreneur or ruined proletarian, he did not deny, but attached no significance to, the fact that the majority of the population belonged to neither group. After reading several statistical studies that documented inequalities within the commune, Lenin made the astonishing claim that in 1893 rural Russia was predominantly capitalist.<sup>54</sup>

Lenin's major work on Marxist economics, *The Development of Capitalism in Russia* (1899), was a continuation of his antipopulist polemics. His research contained serious flaws, some excusable, others less so. Like Plekhanov, he found proof in the *zemstvo* investigations of "precisely the social-democratic conception of reality, the conception of the peasant as petty bourgeois."<sup>55</sup> Although he was concerned above all with the accelerating process of rural differentiation as it was reflected in statistics on peasant ownership of livestock and agricultural machinery, Lenin did not refer to contemporary statistics that dealt with changes in the peasant economy over time. He attempted instead to demonstrate the *dynamics* of differentiation by using static figures (data, for example, that refer to the level of economic inequality in 1893). Lenin ridiculed the populist P. N. Vikhliaev's study of peasant farming for its emphasis upon egalitarian tendencies within the commune but did not mention Vikhliaev's statistics on the pattern of peasant ownership of horses, perhaps because these figures did not demonstrate the growth of rural differentiation. Lenin was then able to conclude that the rural proletariat was growing at a great rate and that the rural bourgeoisie were already the "masters" of the Russian countryside.<sup>56</sup>

Within the terms of Lenin's analysis, even the assertion that most Russians

<sup>53</sup> According to Lenin's definition, "romanticism takes as a model the association that satisfied the narrow need for organization in a patriarchal, immobile society and wants to apply it to a totally transformed society"; *Polnoe sobranie sochinenii*, 2: 528.

<sup>54</sup> Lenin, *Polnoe sobranie sochinenii*, 1: 260, 214, 239-40, 489. After 1905, Lenin admitted that he was wrong on this issue, but he never conceded that the economists whose evidence he had rejected had ever been correct; see Esther Kingston-Mann, "A Strategy for Marxist Bourgeois Revolution: Lenin and the Peasantry, 1907-1916," *Journal of Peasant Studies*, 7 (1979-80): 134.

<sup>55</sup> In Lenin's words, the breakup, the "depeasantization" of the countryside, was established beyond question "thanks to the admirable material provided by *zemstvo* statistics"; *Polnoe sobranie sochinenii*, 1: 237-38.

<sup>56</sup> Lenin, *Polnoe sobranie sochinenii*, 3: 135.



were peasants living on inadequate allotments of land became a matter for debate. According to Lenin, the advance of capitalism was rapidly filling the Russian countryside with landed "proletarians" who were so poor that they "really" had nothing to lose but their chains. Poverty was "proletarianizing" the peasants even before their social relations precisely conformed to the Marxist definition of the term.<sup>57</sup> Here and elsewhere, Lenin's mode of classification was extraordinarily flexible. At times he divided the peasantry into antagonistic classes according to their ownership of land, livestock, tools, or use of hired labor, but he did not consistently use any one of these indicators as a basis for his division of the peasantry into kulaks and proletarians. As the Polish sociologist Boguslaw Galeski has suggested, no category corresponded strictly either to the peasant stratum as a whole or to any group within it.<sup>58</sup> In 1899, Lenin was quite willing to argue that peasants on tiny allotments of land were essentially proletarian in an economy dominated by the rural bourgeoisie. After 1905, he put forward with equal confidence the claim that small allotments sustained a system of *feudal* relationships between the peasantry and the landed gentry.<sup>59</sup>

But even in the 1890s, Lenin did not claim that all of the rural poor were proletarians. He recognized the importance of the "serflike" peasantry, and his research on the survivals of feudal obligation was (perhaps unsurprisingly) far more solid and convincing than his assertions on the triumph of capitalism. According to Lenin, the emancipation of 1861 had created new forms of feudal dependence by imposing onerous financial burdens on the peasantry, by diminishing allotment size, and by restricting freedom of movement. Existing inequality was thereby increased, without establishing the conditions for the most rapid and efficient transformation of peasants into kulaks and proletarians. Where serfs had worked without compensation on the lord's land using their own tools, "free" peasants on inadequate allotments of land now farmed the gentry estates under the same conditions in return for the use of *otrezki* lands taken from them in 1861.<sup>60</sup> The feudal *barshchina* had become the *otrabotka* system; the *otrezki* guar-

<sup>57</sup> It is useful to consider Lenin's argument in the context of Georg Lukacz's later explorations of the disjunction between "objective" economic conditions and "proletarian" consciousness; see Lukacz, *History and Class Consciousness* (London, 1971), 51, 71-75, 81.

<sup>58</sup> Galeski, *Basic Concepts of Rural Sociology* (Manchester, 1972), 107. Galeski has illogically concluded that Lenin's approach nevertheless provides "a basis for determining the relative weight of class groups in the peasant stratum and for analyzing the genesis of the Russian rural structure"; *ibid.*

<sup>59</sup> In both cases, economic analysis seemed to give way to the need for a politically constructive peasant policy. In the 1890s, a potentially revolutionary rural proletariat could be calculated so as to contain almost half of the peasant households in Russia; in the 1900s, when (for complex reasons) Russia regressed in Lenin's writings to a somewhat earlier economic stage of development, Lenin claimed the allotment peasantry was a major force in the struggle against a feudal economic order. See, for example, Lenin, "Agrarnyi vopros k kontsu XIX veka," (1908), in *Polnoe sobranie sochinenii*, 17: 72-78, 84. Even in the 1890s, however, when Lenin claimed that the allotment peasant was a proletarian, he argued against the populist V. P. Vorontsov that destitute proletarians constituted a sufficient domestic market for Russian infant industry because they were forced to purchase everything they needed in order to survive. Vorontsov believed that the separation of industry from agriculture had ruined the Russian peasant, whose income was partially derived from handicraft production. In contrast to Lenin, Vorontsov insisted that a peasant unable to pay taxes or purchase tools could provide for Russian industry neither a domestic market nor a major source of capital. See V. P. Vorontsov, *Sud'ba kapitalizma v Rossii* (St. Petersburg, 1882), 108-17.

<sup>60</sup> According to the terms of the emancipation, the pasture and forest lands traditionally used by the peasants were made the property of the former serfowners. These were the *otrezki*, or "cut-off lands."

anted the peasantry's continued dependence upon the landed gentry. Since the emancipation had bound the liberated serfs to the commune and the communal allotment, they were unable either to compete freely with other proprietors or to sell their labor at will as rural proletarians. Lenin argued, therefore, that a free labor market would destroy the bonds of the *otrabotka* system and liberate the peasantry once and for all from the isolation and stagnation of the village community.

In "backward" Russia, the operation of the market would prove to be far more educational than any schools that enlightened reformers might devise. In a statement that could have come from S. Iu. Witte, the "state capitalist" finance minister of Nicholas II, Lenin wrote that it was "naive to imagine that a village school can teach people what they can learn from an independent acquaintance with the differing relations and order of things in agriculture and in industry."<sup>61</sup> Peasants would be "civilized" by the experience of poverty in a more stimulating environment. Freed from the patriarchal dependence and mindless routine of rural social life for the presumably varied work to be found in the Russian factory, peasants and their children would achieve the level of class consciousness that only the socialized production process of industrial capitalism could generate.<sup>62</sup> Taking pride in his Marxist humility, Lenin distinguished himself from the populist intellectuals who claimed to possess "super-human power" to choose "more or less favorable paths" for Russia's future development. Lenin argued that Marxists were truly scientific because they could accept evidence demonstrating that the economic laws Marx had discovered applied not only to England but to Russia as well.

In general, Lenin's work provided a far more complex and politically insightful analysis of Russian rural life than Plekhanov's. Stressing always the social element upon which a revolutionary struggle might be based, Lenin categorized the awkward majority of peasants who neither employed labor nor hired themselves out to work for others either as serflike peasants who might be induced to struggle against feudalism or as some sort of potentially revolutionary proletariat. And, although Lenin sometimes gave in to the temptation to de-emphasize the "capitalist injustices" that so concerned the populists, it was more characteristic of him to revel in the hard "truth" that capitalism was at once a cruel and a civilizing phenomenon.<sup>63</sup> Insistent on the link between capitalist

<sup>61</sup> Lenin, *Polnoe sobranie sochinenii*, 3: 246. While Witte rested his hopes on sturdy entrepreneurs rather than rebellious proletarians, both Lenin and Witte considered capitalism an unsurpassed form of schooling. As Witte wrote in 1899, "no matter how significant the promotion of enlightenment, that road is too slow. . . . The natural school of industry is first of all a lively industry. . . . The first investment of savings awakens in man the restlessness of enterprise, and with the first investment the powerful stimulus of personal interest calls forth such curiosity and love of learning as to make an illiterate peasant into a railway builder, a bold and progressive organizer of industry. . . ." Witte, "An Economic Policy for the Empire," *Readings in Russian Civilization*, ed. Thomas Riha, 2 (Chicago, 1968): 434.

<sup>62</sup> Lenin, *Polnoe sobranie sochinenii*, 3: 549.

<sup>63</sup> See note 58, page 747, above. It is worth considering whether hard truths are always more reliable than the truths of the "tender-minded." Vorontsov's argument that Russia lacked a domestic market for her industry and would be unable to compete with her more advanced Western capitalist rivals may go further than the predictions of Lenin and Witte to explain why the economic gap between Russia and Germany, Japan, and the United States (estimated in terms of per capita growth) was larger in 1913 than it was in 1861. See R. W. Goldsmith, "Economic Growth of Tsarist Russia, 1860-1913," *Economic Development and Cultural Change*, 9 (1961): 442-43, 474-75.

brutality and progress, Lenin did not formulate an economic alternative to massive peasant ruin in the modernization process.<sup>64</sup>

BY THE LATE NINETEENTH CENTURY, Plekhanov and Lenin had decided that the commune was a bankrupt and reactionary institution. Unwilling to confront the insights that Marx had praised in the work of Daniel'son, they ignored the social relations of peasant agricultural production except as those could be fitted into economic categories derived from Western European experience. "Backward" peasants were therefore described as serflike dependents of the gentry and the state, whose relations to one or another had no meaningful structure; "modern" peasants were proto-capitalist, defined by their participation in class conflict over the issues of wages, property, and profit.

Such an approach yielded Marxists some important insights and a host of misunderstandings. The Marxist perspective brilliantly exposed the myriad disadvantages of precapitalist village life, where peasant solidarity might erupt in pogroms against the unfortunate outsider. Sensitive to the manifestations of rural capitalism, Plekhanov and Lenin were able to see that the commune could disguise kulak domination of the poor. They sought and found kulak and proletarian tendencies in the numerically dominant "middle" peasants who neither hired labor nor hired themselves out to work for others. As Marxists, they were able to identify the new forms of dependence imposed by the landed gentry upon the peasantry and to describe with clarity and precision the capitalist process that destroyed the survivals of such "feudal" obligation.

From the Western-centered perspectives of Plekhanov and Lenin, however, the majority of the population became unimportant in comparison to the more "dynamic" kulak and proletarian minorities. This skewed emphasis became a permanent peculiarity of the Russian Marxist approach to rural development, despite the fact that most peasants were neither kulaks nor proletarians in the 1890s when Lenin wrote, in 1905 when the tsarist regime began to promote rural capitalism, and even in 1918 when the Soviets launched their abortive campaign to organize proletarians against kulaks in the "Committees of the Poor." In *The ABC of Communism* (1918), a classic economic and political text of the Civil War period, N. I. Bukharin and E. A. Preobrazhenskii described the peasants as petty proprietors at a time when the majority still did not own land or hire labor.<sup>65</sup> As Marxists, they insisted that the peasantry lacked historically significant forms of social organization, although the commune survived and even increased its power and influence as a land-equalizing mechanism until it was

<sup>64</sup> A strategy of sorts did emerge in the period after the revolution of 1905-07; see Kingston-Mann, "A Strategy for Marxist Bourgeois Revolution: Lenin and the Peasantry, 1907-1916," 132-57. Other Russian Marxists who wrote on the problems of rural development were far more procapitalist than either Lenin or Plekhanov. The "legal Marxists" S. M. Bulgakov, P. B. Struve, and M. I. Tugan-Baranovskii, fanatic Westernizers all, eventually discarded socialism as a future goal. See, for example, Bulgakov, *Kapitalizm i zemledelie*, 1 (St. Petersburg, 1900): 269-83; Struve, *Kriticheskie zametki k voprosu ob ekonomicheskom razviti Rossii* (St. Petersburg, 1894), 159-60; and Tugan-Baranovskii, *Promyshlennye krizisy: Ocherki iz sotsial'noe istorii Anglii* (St. Petersburg, 1900), 30-31.

<sup>65</sup> Bukharin and Preobrazhenskii, *The ABC of Communism*, trans. Eden Paul and Cedar Paul (Ann Arbor, 1966), 316-20.

forcibly destroyed by the Soviet government in the period after 1929.<sup>66</sup> Before and after the revolution, they conceived of rural change as a one-way process, rejecting as "sentimental" the suggestion that peasants had any economic or social experience to contribute to the building of modern society. Such judgments, based upon a rejection of the conclusions and much of the evidence in the economic works they cited, shaped the Marxism of Bolsheviks and Mensheviks alike in the twentieth century. When revolutions occurred "prematurely" in 1905 and 1917 and the autocracy embarked on a modernization policy based upon the destruction of the commune, Marxists in the grip of such assumptions were left to choose either to manipulate or to coerce an "obsolete" peasant majority.<sup>67</sup>

Their dilemma was not simply a product of the facts of Russian life. The evidence provided by *zemstvo* investigations and by the research of academic and nonacademic economists in the last two decades of the nineteenth century indicates that communes were neither moribund, wholly controlled by the rich, nor resistant to economic progress and that peasant commitment to traditional guarantees of security in times of escalating external pressure was not a conclusive proof of natural conservatism. The Marxist decision to focus upon the purely economic behavior of a tiny minority of kulaks and proletarians owed much to a vision of Western culture that had steadily become more simple and schematic since the first debates of the Slavophiles and the Westernizers of the 1840s.<sup>68</sup>

During the course of the nineteenth century, educated Russians found it increasingly difficult to argue for a selective use of Western "lessons" and "models," as each decade brought new and devastating proof that the world belonged to a small number of Western European imperial states. Western material achievements were rightly recognized as historically linked with a capitalist transformation of village and town, the evolution of modern science, and the emergence of a powerful urban middle class whose culture came to define the meaning of progress for all Europeans and non-Europeans who wanted to become part of the civilized world. Russian radicals had learned from a generation of liberal European historians of courageous bourgeois efforts to destroy a feudal world in which idle nobles prospered and ignorant peasants clung to medieval guarantees of a wretched material existence. In the West, a rather simplistic view of the peasantry as a conservative force and the middle class as a fundamentally progressive influence flourished in the nineteenth century and received

<sup>66</sup> See Moshe Lewin, *Russian Peasants and Soviet Power* (London, 1968), 133.

<sup>67</sup> Teodor Shanin, *The Awkward Class: Political Sociology in a Developing Society—Russia, 1910–1925* (London, 1972), 160–62.

<sup>68</sup> Both Slavophiles and Westernizers were "Westernized" Russians, convinced of the benefits of European science, technology, and culture. A. S. Khomiakov's vision of a renewed and revitalized "traditional" Russia did not exclude modern Western science and industry; V. G. Belinskii's admiration for Western political and cultural achievements did not keep him from denouncing bourgeois greed and calculation with the passion of a medieval schoolman. For a useful discussion of Khomiakov's views, see P. K. Christoff, "A. S. Khomiakov on the Agricultural and Industrial Problem in Russia," in Alan Fergusson and Alfred Levin, eds., *Essays in Russian History* (Hamden, Conn., 1964), 144–45; and, for Belinskii's views, see his *Izbrannye filosofskie sochinenii*, 2 (Moscow, 1948): 165–70.

the sanction of the social science of the new age.<sup>69</sup> In Russia, such judgments formed part of the conventional wisdom of the most tough-minded adherents of the Left and Right.

There were, of course, other Western traditions and ideological models; certainly, educated Russians of every non-Marxist ideological persuasion understood the modernization of Europe as an extremely varied process.<sup>70</sup> But, for Russian Marxists engaged in the struggle against populist subjectivism, "Europe" contained only two vital, rational forces—a progressive, liberal, profit-seeking bourgeoisie and an exploited, numerous, and revolutionary proletariat. By the last decades of the nineteenth century, any insistence upon the primacy of Russian conditions for Russian development came to appear hopelessly naive. As Plekhanov wrote disdainfully of Mikhailovskii, "he has fallen behind intellectual progress."<sup>71</sup> Within the context of Lenin's untenable argument that the bourgeoisie were already the masters of the Russian countryside, populist attempts to "resurrect" the tie between the peasants and the land appeared particularly quixotic. "How foolish the populists were," Lenin wrote, to dream that they could simply "pluck desirable elements from a variety of historical formations—the unity of producer and the means of production from the medieval order and the Enlightenment, freedom, and culture from capitalism."<sup>72</sup>

Like many of their ideological opponents, Lenin and Plekhanov accepted the necessity for economic development at whatever cost to the peasantry. They accepted as well the modernizers' claim to unlimited sovereignty over the bodies and resources of the peasantry and the corollary right to exclude the peasantry's collective experience as irrelevant to the process of rural transformation. Indifferent to what most peasants, because of a shared past, were and might become in common, Lenin and Plekhanov were, for all of their historical determinism, profoundly unhistorical. In their certainty that Russia must follow the "English path," they did not entertain the hypothesis that indigenous rural values and institutions could evolve or might even be in the process of evolving. Clearly, they did not consider the latter possibility worth the sort of close investigation re-

<sup>69</sup> In the course of the nineteenth century, the image of the peasant seemed to fluctuate back and forth between "apathetic" and "stupidly greedy and competitive" in the writings of Marxist and non-Marxist modernizers. These shifts led to changes in strategy that weighed heavily on an already burdened peasantry. For a discussion of this general problem, see Esther Kingston-Mann, *The Burden of Western Wisdom: Western Models for Russian Rural Progress, 1861–1928* (forthcoming).

<sup>70</sup> A number of the most influential of Russian economists—among them A. I. Chuprov—studied with Wilhelm Roscher of the German "historical" school of economics. Roscher, like Adolf Wagner and Gustav Schmoller, were critical of Marx and his "classical" predecessors because they excluded questions of social cost from their economic analyses. According to Roscher, the English process of "primitive accumulation" imposed burdens on the general populace that were unacceptable in human terms. In line with such arguments, Chuprov, like A. S. Posnikov and V. P. Vorontsov and unlike N. I. Ziber, called for a re-integration of questions of distribution and consumption into the production-oriented economics of the English school. Roscher's suggestion that rational agricultural economies were based upon a mixture of scale and forms of tenure was particularly influential in this connection. See, for example, Roscher, *Nationalökonomie des Ackerbaues* (Leipzig, 1885), 65–67.

<sup>71</sup> Plekhanov, *Sochineniia*, 6: 53.

<sup>72</sup> Such arguments were less than compelling coming from Lenin, whose analysis of Russian agriculture was completely devoid of discrete historical formations, whether medieval or capitalist. Lenin, *Polnoe sobranie sochinenii*, 1: 191–93.



quired by questions of productivity and profit. Instead, dogmatic Marxists found rural tradition inflexible and unchanging, and Plekhanov boasted that he would make of *Das Kapital* a "Procrustean bed" for the populists.<sup>73</sup> His real achievement may, in fact, have been to perform that function for Russian Marxists. In the last decades of the nineteenth century, a culture of modernization had taken shape, a culture that justified any action to render the peasantry obsolete long before "history" or the laws of economic development had succeeded in doing the job.

<sup>73</sup> Plekhanov, *Dela i dni: Istoricheskii zhurnal*, 2 (1921): 91, as quoted in Samuel H. Baron, *Plekhanov: The Father of Russian Marxism* (Stanford, 1963), 77.

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## The Tsarist Officer Corps, 1881–1914: Customs, Duties, Inefficiency

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JOHN BUSHNELL

TWO BEST-SELLING NOVELS OF RUSSIAN MILITARY LIFE illustrate the problem: in the years before World War I, tsarist officers sustained patterns of behavior incompatible with their military mission. The publication of Aleksandr Kuprin's *Poedinok* (*The Duel*) in May 1905 caused a literary and political sensation. Kuprin drew an unrelievedly bleak picture of the lives of tsarist officers in a small provincial garrison in the 1890s. Almost without exception, the officers in the novel are given to bouts of solitary or collective drunkenness, and each has, in addition, his own personal vice. One cheats at cards, another despoils teen-aged girls, others steal from their men or from company funds. All are bored to death (literally in some cases) of military service, but they lack the talent to earn an honest civilian living and hold civilians in contempt. Only the fear of disgrace at the annual review rouses them from cynical indifference to their vocation: in the weeks preceding they beat their men (again literally) into a suitable parade-ground semblance of military efficiency. Apart from the review, only drink, cards, scandal, and violence punctuate the tedium of the officers' lives. Appearing as it did in the midst of the Russo-Japanese War and the 1905 revolution, *The Duel* was read as an explanation of the recent failures of the tsarist army and a radical critique of the principal institution propping up the tsarist regime—an indictment, as one reviewer put it, “of the existing order of things,” of “bureaucratic [that is, autocratic] carrion.” By the end of the year, more than forty thousand copies had been sold.<sup>1</sup>

*The Duel* fortuitously addressed the two great issues of the day. Kuprin had begun work on the novel in 1902, before either war or revolution, and it was as much a literary reworking of his own, obviously unhappy, military experience as a social exposé. Kuprin had served as a junior officer in just such a provincial garrison as that in which *The Duel* was set, and his familiarity with military life lent force to his characterizations of the tsarist officer corps. Although Kuprin's

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<sup>1</sup> V. Afanas'ev, *Aleksandr Ivanovich Kuprin* (Moscow, 1972), 54; and A. I. Kuprin, *Sobranie sochinenii*, 6 vols. (Moscow, 1957–58), 3: 302–541. For surveys of the immediate critical reaction to *The Duel*, see Afanas'ev, *Kuprin*, 53–56; and F. I. Kuleshov, *Tvorcheskii put' Kuprina* (Minsk, 1963), 279–84.

collective portrait amounted to parody, a few officers were prepared to concede that the novel contained "no little melancholy truth."<sup>2</sup>

More telling than such grudging admissions that Kuprin had drawn his characters from life are the striking parallels between the mores described in *The Duel* and those depicted in a novel of a different stripe. General Petr Krasnov's *Ot Dvuglavogo Orla k krasnomu знамени* (*From Double Eagle to Red Flag*), published in Berlin in 1921 and a runaway bestseller in the Russian emigré community, traces the fortunes of tsarist guards officers from the 1890s through the Civil War, in which Krasnov himself was one of the early anti-Bolshevik leaders. Krasnov certainly did not intend to condemn tsarist officers; he recalled a vanished society, with some sorrow for its failings but with much affection as well. Yet Krasnov's opening scene—an all-night party (wine, women, and cards) followed by cancellation of morning drill—might have been written by Kuprin. In another scene, drunken officers led by their commander stage a mock drill around the mess, crawl under the table on the command, "There are no gentlemen," and re-emerge on the command, "Gentlemen exist." Krasnov did not consider these and similar incidents above reproach, but neither narrative tone nor authorial comment indicates that they were out of the ordinary. The officers in *Double Eagle* are, for the most part, either indifferent to their military duties or empty-headed. During summer exercises they picnic to avoid encumbering their men; indeed, Krasnov's officers prefer to take leave during summer maneuvers. In the years before the Russo-Japanese War, the officers in *Double Eagle* make no effort to prepare themselves or their men for war, and most persist in the old routine through 1914. During the Great War itself, gallant death redeems them.<sup>3</sup>

The similarity of the social customs and duty behavior that Kuprin and Krasnov described is remarkable, if only because the two groups of officers were in other respects quite different: Kuprin's world was the humble line infantry, Krasnov's the socially exclusive guards cavalry. More interesting still is that this officer lifestyle was set in the period following the far-reaching military reforms sponsored by Dmitrii Miliutin (minister of war, 1861–81). These reforms—administrative reorganization, rearmament, the upgrading and reorganization of officer training, and the introduction of nearly universal military obligation, to name only the most important—sought to remedy the defects exposed during the Crimean War, and historians have generally credited them with trans-

<sup>2</sup> Drozd-Boniachevskii denounced the composite portrait Kuprin offered as calumny but undertook a detailed critique of the tsarist officer corps, illustrating each point with examples from *The Duel*; Drozd-Boniachevskii, "Poedinok' Kuprina s točki zreniia stroevogo ofitsera," *Voennyi sbornik*, no. 1 (1910): 172, and no. 2 (1910): 175–88. General A. I. Denikin's retrospective assessment was that, "although each character in *The Duel* was drawn from life, there was no such collection of characters, no such regiment in the Russian army"; Denikin, *Staraiia armia*, 2 vols. (Paris, 1929): 1: 128–29. For Kuprin's military education and service and his writing of the novel, see Kuleshov, *Tvorcheskii put' Kuprina*, 14–48, 207–29. Many of Kuprin's short stories draw on his military experience, and two of his works, "Na Perelome (Kadety)" (1900) and *Iunkera* (serialized in *La Renaissance*, Paris, 1928–33), are frankly autobiographical accounts of his life in military schools.

<sup>3</sup> P. N. Krassnoff, *From Double Eagle to Red Flag*, trans. Erik Law-Gisiko (New York, 1928); for the public reception of Krasnov's novel, see Gleb Struve, *Russkaia literatura v izgnaniiu* (New York, 1956), 125–26.

forming the tsarist army into at least an approximation of a modern, professional force.<sup>4</sup> Yet, if the officers described by Kuprin and Krasnov were representative, something fundamental was amiss in the post-Miliutin army.

Officers who entered the service or rose to command rank between 1881 and 1914 led Russia to defeat in the Russo-Japanese War and World War I, and a study of their peacetime behavior should illuminate the preparation, or lack thereof, that went into those defeats. It may also shed light on the reasons for the decline in tsarist military prowess that began early in the nineteenth century. Officers ought not, of course, to be saddled with all of the responsibility for Russia's later military incapacity; Russia waged war under social, economic, and geographic handicaps, although these had not precluded victory in the eighteenth and early nineteenth centuries. Ultimately, the structure of the nineteenth-century tsarist polity was at fault, but an explanation of military failure couched in those terms is rather distant from the reality of combat. The battlefield tested most directly the army's professional competence, and that leads back to the officer corps. As General Mikhail Dragomirov, renowned for high training standards and pungent aphorisms, liked to say, "A fish stinks from the head."

AS THE ACCOUNTS OF TSARIST OFFICER LIFE in *The Duel* and *Double Eagle* imply, drunkenness was widespread and heavy drinking the norm. Without access to mess books, we cannot quantify per officer consumption of alcohol, but the evidence of the memoirs is unambiguous. There is literally no officer memoir, published or unpublished, that does not report either individual cases of drinking to incapacitation (with no negative consequences for the officer's career) or a uniform pattern of heavy drinking. Some émigré memoirists, sensitive on this point, asserted that heavy drinking was infrequent and that the cases they reported were exceptional. When every memoir has such exceptions to report (and when it was felt necessary to raise the matter in the first place), it is clear that they were the rule.<sup>5</sup> Almost invariably, the memoirs attribute heavy drinking to the peculiar environment of the units in which it was reported. Officers in Siberia were notorious drunks because there was nothing else to do. Officers in Poland

<sup>4</sup> For details of the Miliutin reforms and the politics behind them, see P. A. Zaionchkovskii, *Voennye reformy 1860-1870 godov v Rossii* (Moscow, 1952); and Forrest A. Miller, *Dmitrii Miliutin and the Reform Era in Russia* (Nashville, Tenn., 1968). Neither study addresses the critical question of the effect of the reforms on military efficiency, although Zaionchkovskii has discussed the poor performance of the tsarist army during the Russo-Turkish War of 1877-78. A. A. Kersnovskii made some suggestive remarks on the excessive bureaucratization of the army and the continued inefficiency of the officer corps in this period; Kersnovskii, *Istoriia russkoi armii*, 3 vols. (Belgrade, 1933-38), 2: 397-411.

<sup>5</sup> Two of many contemporary accounts may serve as examples. V. N. Fon Dreier reported that at the turn of the century drinking was not widespread in the Tashkent garrison but that in his battery the commander drank from noon till late at night, that another officer beat up soldiers when he was drunk, and that two other officers drank "infrequently"—only two or three times a month, each bout lasting a week; Fon Dreier, *Na zakhate imperii* (Madrid, 1965), 18-20. And Pavel Il'ich Baranovskii reported that, when visiting a regiment near the western border shortly before World War I, he was momentarily confused when mess attendants as a matter of course served cognac in place of tea at breakfast; Baranovskii, "Vospominaniia" (typescript; n.p., n.d.), 61, Bakhmeteff Archive, Columbia University [hereafter BA].

drank inordinately because units were based, for strategic purposes, far from major cities. Over the length and breadth of Russia's provinces, officers drank because they were in boring provincial towns and outposts.<sup>6</sup> Such special circumstances, then, were in reality the norm. The truly exceptional circumstance not conducive to drunkenness was, according to contemporary opinion, a posting in one of the few large, cosmopolitan cities: fathers in military families often instructed their sons to join regiments in big cities, where entertainment could be found outside the mess. But there was much drinking even in the big cities. Officers in the elite St. Petersburg guards regiments and the fashionable Moscow regiments staged regular all-night, regimental drunks and routinely drank to excess in restaurants.<sup>7</sup>

The evidence certainly does not indicate that most tsarist officers were habitually drunk, but it does show that drinking was a standard component of the officers' daily routine. It is, therefore, not surprising that officers scarcely noticed the genuine alcoholics in their midst or that official efforts to combat alcoholism were ineffective. In May 1906 the Ministry of War prescribed measures against officer alcoholism (not because alcoholism impaired efficiency but because it undermined the officers' authority in a period of rampant mutiny), and an imperial order of 1908 decreed that drunkenness cease and alcoholics be discharged. Yet, when Krasnov took command of a Cossack regiment in Central Asia in 1911, he found that one of his officers was a certifiable alcoholic and dismissed the man from service. (Preceding commanders had provided the alcoholic with seven orderlies because his family could not otherwise have coped.)<sup>8</sup> In a subsequent command Krasnov banished vodka from the mess. But Krasnov's campaign against drinking was exceptional. Everywhere the officer who did not drink was considered odd, drinking to stupefaction was part of the initiation ritual for young officers newly posted to a regiment, and junior officers who drank but moderately were likely to be reproved by their commanders. So strongly entrenched was officer inebriation that not to drink was, under certain conditions, taken as an act of political deviance. A monarchist officer recalled that during the turmoil of 1905 "revolutionary" officers ostentatiously shunned vodka, while monarchists "served conscientiously, drank conscientiously," gambled, and oth-

<sup>6</sup> N. Voronovich, *Russko-iaponskaia voina: Vospominaniia* (New York, 1952), 9; B. V. Gerua, *Vospominaniia o moei zhizni*, 1 (Paris, 1969): 115, 201; Denikin, *Staraiia armiiia*, 1: 44; A. Gerua, *Posle voiny: O nashei armii* (2d edn., St. Petersburg, 1907), 81; Aleksandr Bragin, "Vospominaniia: Zhizn' mysh'ia begotnia," 2 vols. (typescript; Paris, 1941), 1: 6-12, 106, BA; and A. Lobanov-Rostovsky, *The Grinding Mill: Reminiscences of War and Revolution in Russia, 1913-1920* (New York, 1935), 168-69. The extremely demoralizing life in provincial garrisons produced such popular games as "cuckoo," in which officers took potshots at the sound of each other's voices in darkened rooms. The game thrived in the early twentieth century despite the efforts of the authorities to put an end to it; Paul Rodzianko, *Tattered Banners: An Autobiography* (London, 1939), 111.

<sup>7</sup> Dmitrii L. De-Vitt, "Tritsat'piat' let tomu nazad" (typescript; Paris, 1950), 1, BA; B. V. Gerua, *Vospominaniia*, 63-64; A. A. Ignatyev, *A Subaltern in Old Russia* (London, 1944), 93-94; F. N. Buak, "Vospominaniia starogo kavalergharda 1885-1902 goda" (typescript; n.p., n.d.), 22-23, 83-84, BA, MSS Coll/Rozen, box 2; Rodzianko, *Tattered Banners*, 109-110; and Vladimir S. Littauer, *Russian Hussar* (London, 1965), 20-21.

<sup>8</sup> "Dokumenty moskovskogo voennogo okruga, 1903-1906" (typescript; n.p., n.d.), International Institute for Social History, Amsterdam, SR Archive, dossier 690; V. A. Zambrzhitskii, "Ocherki bylogo: Vospominaniia ofitsera General'nogo Shtaba (Iz tsikla sobytii 1-i mirovoi voiny)" (typescript; n.p., 1954), 21, BA; and P. N. Krasnov, *Na rubezhe Kitaia* (Paris, 1939), 61.



erwise comported themselves in a way befitting proper officers.<sup>9</sup> Heavy drinking, then, was in no way considered incompatible with the proper performance of duties; it was a prescriptive norm and, if necessary, was learned.

Theft, misappropriation, and graft were also acquired social norms. At the least, officers learned by example that they could get away with theft. Company commanders stole from their men either by not handing over pay (this was considered risky because it inevitably became public knowledge) or by helping themselves to the money that soldiers' families sent their sons to defray the costs of service.<sup>10</sup> Everyone in charge of purchasing and supplies had an opportunity for misappropriation and graft, and a large proportion of officers profited in this way.<sup>11</sup> The higher the rank and the more responsible the position, the easier it was to steal. General staff officers of the Moscow military district, led by the district chief of staff, pocketed the money allocated for a military statistical survey in the early 1900s; Russian commanders during the Boxer campaign converted discrepancies in exchange rates into personal profit; corps commanders who received large sums to meet expenses at the outset of the Russo-Japanese War did not return the money when they were replaced before their units had set out for the theater; ranking officers took advantage of reimbursement procedures for official travel and turned a profit of hundreds, even thousands, of rubles annually.<sup>12</sup> Tsarist officers knew that theft was possible; they also knew that it would not be punished unless they were very incautious indeed.

Unlike drinking, officer theft was formally deviant behavior, and it was not flaunted. Nevertheless, the available evidence indicates that it was much more than episodic. Theft had a venerable lineage in the tsarist army, and the tradition remained vital to the last for the good reason that it was functional. Officers were poorly paid, and married officers especially found their financial circumstances difficult. Theft, however, was the complement less of low pay than of the expenses necessary to maintain the proper officer lifestyle—regimental dinners, fine wines in the mess, flowers for the ladies of the regiment on all appropriate occasions, and so on. Drinking and gambling debts, which had somehow to be

<sup>9</sup> V. N. Birkin, *Osinoe gnezdo: Povesti minuushikh let*, 4 (Berlin, 1930): 89-90; Artur Georgievich Bitenbinder, "Vospominaniia" (typescript; n.p., n.d.), chap. 4, pp. 5-6, BA; and Rodzianko, *Tattered Banners*, 109-10.

<sup>10</sup> *V tsarstve shtykov* (Nizhnii Novgorod, 1908), 13, 23; P. N. Krasnov, *Nakanune voiny* (Paris, 1937), 14-15; and Bragin, "Vospominaniia," 1: 33-34, 64-66. Also see the article by "Ofitser" in *Iskra*, March 10, 1905. On the practice of parents sending soldiers money and the reasons for it, see John Bushnell, "Peasants in Uniform: The Tsarist Army as a Peasant Society," *Journal of Social History*, 13 (1980): 567-68.

<sup>11</sup> Bragin, "Vospominaniia," 2: 66-67; G. Mannerheim, *Erinnerungen* (Zurich, 1952), 20-21; S. A. Tsion, *Tri dnia vosstania v sveaberge* (Helsingfors, 1907), 15, 17-18; M. Sil'vin, *V kazarme: Iz nabliudeniĭ sots.-demokrata* (Geneva, 1903), 9-10; Petr Pil'skii, "Armiia i obshchestvo: Elementy vrazhdy i prepiatstvii," *Mir bozhii*, no. 7 (1906): 223-24; F. Kriukov, "Armeiskaia didaktika," *Russkoe bogatstvo*, no. 3 (1912): 152; M. Grulev, *Zloby dnia v zhizni armii* ([Brest-Litovsk] 1911), 23; V. Sukhomlinov, *Vospominaniia* (Berlin, 1924), 94-95; and Krasnov, *Na rubezhe Kitaia*, 66.

<sup>12</sup> M. Grulev, *Zapiski generala-evreia* (Paris, 1930), 230-31, 234; Bragin, "Vospominaniia," 1: 74-75; P. I. Zaleskii, *Vozmezdii: Prichiny russkoi katastrofy* (Berlin, 1925), 63; and Unsigned letter to "M. Gospodin Aleksinskii," 1907, BA, Aleksinskii Papers, box 6, correspondence folder—Second Duma, 11. Minister of War Sukhomlinov was notorious both for taking unnecessary trips to collect travel pay and for accepting bribes. His behavior was scandalous because of its visibility, not because it was abnormal; Walter Thomas Wilfong, "Rebuilding the Russian Army, 1904-1914: The Question of a Comprehensive Plan for National Defense" (Ph.D. dissertation, Indiana University, 1977), 111-12.

met, were not the least item in the officer budget. Guards officers, however, apparently did not avail themselves of the opportunity for petty theft and graft, since their obligatory social expenses were so great that only officers with an outside income of one hundred or more rubles per month gained admittance to guards regiments. (Command-rank grafters did include officers of guards background.) Impecunious line officers could meet their social expenses only illicitly.<sup>13</sup> Although features of the regimental economy provided a suitable framework for and some training in officer theft, there was nothing in the economic, still less in the military, functions of the tsarist regiment that mandated theft of the personal sort under consideration here. Theft was bound up in the social behavior of the officer corps.

The social endogeneity of the officer corps facilitated the transmission of such behavioral norms. Observers often noted the tendency of tsarist officers to spend their time exclusively in each other's company (a tendency certainly not unique to the tsarist army). One's fellow officers alone set the standards for appropriate behavior. Two-thirds of the officers were bachelors, which likewise contributed to the homogeneity of social mores (married officers might live a life apart from the mess; bachelors did not). Junior officers, whose socialization was crucial to the maintenance of custom, were especially likely to be single, because tsarist military regulations proscribed marriage before age twenty-eight.<sup>14</sup> A crude caste mentality augmented the exclusivity stemming from social arrangements. Officers saw themselves as a breed apart from civilians, a view they acquired, if nowhere else, in the military schools. Wrote one officer of his newly commissioned brethren, "One not infrequently has occasion to observe how a young man, at even a faint hint that his words are disbelieved, exclaims in a flash of indignation, 'You are being addressed by an officer!'"—in other words, if some-

<sup>13</sup> For general discussions of officer poverty and lifestyle, see Hans-Peter Stein, "Der Offizier des russischen Heeres im Zeitabschnitt zwischen Reform und Revolution, 1861–1905," *Forschungen zur osteuropäischen Geschichte*, 13 (1967): 409–11, 415–18; and P. A. Zaionchkovskii, *Samoderzhavie i russkaia armia na rubezhe XIX–XX stoletii, 1881–1903* (Moscow, 1973), 220–27. Officer memoirs that link officer poverty explicitly with lifestyle include Grulev, *Zloby dnia*, 20–23, 26–27, 168–69; P. Rezhepo, *Neskol'ko myslei po ofitserскому voprosu* (St. Petersburg, 1910), 14–15; K. Smirnov, "Ofitserskii vopros," *Voennyi sbornik*, no. 10 (1909): 135; and Victor Yakhontoff, *Over the Divide* (New York, 1939), 28. On the financial status of guards officers, see B. V. Gerua, *Vospominaniia*, 65, 116, 121; Ignatyev, *Subaltern*, 14–15, 68, 91–92; and N. Voronovich, *Vechernii zvon: Ocherki proshlogo* (New York, 1955), 160–61.

<sup>14</sup> Officers might marry at age twenty-three if they could deposit 5,000 rubles as surety of family support, but very few officers could meet this condition. All marriages required formal approval of the regimental commander, and other officers in the regiment had an informal right to veto one's choice of spouse. A. Rittikh, *Russkii voennyi byt v deistvitel'nosti i v mechtakh* (St. Petersburg, 1893), 136–37; Littauer, *Russian Hussar*, 98; V. A. Zambrzhitskii, "Sapernyi bunt (Popytka k vosstaniu 21-go sapernogo batal'ona v Kieve v 1907 g.): Vospominaniia ochevidtsa" (1953) [31], BA; Stein, "Der Offizier des russischen Heeres," 414–15; and Denikin, *Staraiia armia*, 2 (Paris, 1931): 111. Just after the Russo-Japanese War, twelve of the roughly thirty officers in the Sixth Sapper battalion (Kiev) were married. This may have been an unusually high percentage, because special dispensation was regularly granted to junior officers in this unit; Zambrzhitskii, "Sapernyi bunt" [8, 28]. Just prior to World War I, seven of thirty-six officers in the Horse Grenadiers were married; Voronovich, *Vechernii zvon*, 148. In the First Sumsk Hussars on the eve of World War I, about ten of forty officers were married; Littauer, *Russian Hussar*, 90–91. In His Majesty's Cuirassiers in 1902, twelve of thirty-seven officers were married; N. M. Devlet-Kil'deev, ed., *Kirasiry Ego Velichestva, 1902–1914: Poslednye gody mirovogo vremeni* (Washington, 1959), 14–17, 31. In the 1890s a "large majority" of officers in Her Majesty's Cuirassiers were single; M. A. Svechin, *Zapiski starogo generala o bylom* (Nice, 1964), 29. In two batteries of the Third Guards artillery brigade outside Warsaw in the 1890s, all thirteen officers were bachelors; Ennatskii, "Radimin," *Opoveshchenie po Ob'edineniiu L.-Gu. 3-i Artilleriiskoi Brigady*, no. 13 (July 1935), 15.

thing is said by an officer, no one dare doubt its truth.”<sup>15</sup> The sense of caste that was instilled in the military schools and sustained by corporate tradition involved more than cherishing the honor of the uniform. Military schools provided instruction in the dress and manners that signified good breeding (and added to officers’ expenses). In 1894 duels between officers became legal, and they could be, and frequently were, mandated by courts of honor.<sup>16</sup> Behind this policy of psychological *ennoblement* lay concern over the increasingly base origins of the officer corps. By 1895, barely more than half of all officers were hereditary nobles, and most of these noble officers were of the service rather than the landed nobility (that is, they came from families ennobled for reaching a certain rung in the civilian or military hierarchy). Only a miniscule percentage of noble officers owned property.<sup>17</sup> Cultivation of a caste mentality was functional to the extent that it provided a common social denominator among officers of heterogeneous social origin.

The relative scarcity of wives helped perpetuate traditional officer mores, but it accentuated the importance of the few in circulation. General A. I. Denikin described the activities of “Mother commanders [*mat’-komandirshi*, the partners of the patriarchal *otets-komandira*], who conjointly with their spouses appointed and dismissed, quarreling, making up, marrying, and baptising the military families in their charge.”<sup>18</sup> Others reported that wives at times superseded the authority of their husbands, especially in blocking promotion of officers who neglected to court their daughters at regimental balls or who committed other, more serious indiscretions. Wives below command rank were the social focuses for coteries of bachelor officers, and in one regiment the wives held a formal lottery to allocate arriving officers. Such social groupings could undermine a regiment’s military functions, as in the case of a very junior, but recently arrived, officer who was appointed regimental adjutant because the choice of a more appropriate candidate would have impossibly antagonized one or the other of the matriarchal camps that divided regimental society.<sup>19</sup> The strongest and perhaps strangest

<sup>15</sup> Drozd-Boniachevskii, “‘Poedinok’ Kuprina,” 1: 182. For a fuller discussion of tsarist officers’ political and social thinking, see Peter Kenez, “A Profile of the Prerevolutionary Officer Corps,” *California Slavic Studies*, 7 (1973): 153–57.

<sup>16</sup> Stein, “Der Offizier des russischen Heeres,” 398, 429–35. There were, of course, duels prior to legalization, but they became more frequent after 1894; A. L. Markov, “Bytovie cherty staroi russkoi armii,” *Russkii voenno-istoricheskii vestnik*, 10 (1952): 17–25; Bragin, “Vospominaniia,” 1: 106–07; Buak, “Vospominaniia,” 33–35; Ivanko, “Sluzhba v polku,” in V. I. Shaiditskii, ed., *Na sluzhbe otechestva* (San Francisco, 1963), 268; Denikin, *Staraiia armia*, 2: 84; Peter A. Polovtsoff, *Glory and Downfall: Reminiscences of a Russian General Staff Officer* (London, 1935), 18–23; Rodzianko, *Tattered Banners*, 111–12; and Major General Goncharenko [under the pseudonym Iurii Galich], *Legkaia kavaleriia* (Riga, 1928), 86–92. Regiments were provided with a handbook on duels; V. Durasov, *Duel’nyi kodeks* (4th edn., St. Petersburg, 1912).

<sup>17</sup> Zaionchkovskii, *Samoderzhavie i russkaia armia*, 203, 208–11, 213.

<sup>18</sup> Denikin, *Staraiia armia*, 1: 93. Denikin claimed that by the early 1900s *mat’-komandirshi* were confined to the interior military districts, but his own account mentions a *komandirsha* of an artillery battery in Poland; *ibid.*, 2: 110–11.

<sup>19</sup> Zambrzhitskii, “Sapernyi bunt” [4, 6]; N. D. Butovskii, *Stat’i na sovremennye temy* (St. Petersburg, 1907), 60–71, and *Sbornik poslednykh statei* (St. Petersburg, 1910), 37; Krassnoff, *Double Eagle*, 61–62; Baranovskii, “Vospominaniia,” 53; B. Brazhker, “Otlichno plius otlichno,” *Voennyi sbornik*, no. 2 (1914): 176, 178; P. P. Isheev, *Oskolki proshlogo: Vospominaniia, 1889–1959* (New York, 1959), 72; and Goncharenko, *Legkaia kavaleriia*, 91. A handbook on proper officer behavior for newly commissioned officers provided guidelines on how to treat the ladies of the regiment; Valentin Kul’chitskii, *Sovety ofitseru* (St. Petersburg, 1913), 11–12.

evidence of the key position of wives in the structure of officer society comes from accounts of the Russo-Japanese War. Command-rank wives reportedly accompanied their husbands to the front and rode about just behind the lines; other wives, attached to the medical service, joined their husbands at the line during lulls in the fighting and there commanded the same attentions that they had in peacetime.<sup>20</sup> The rules of *politesse* may have required deference to the ladies (as *noblesse* required the presence of ladies to whom to defer), but social ritual impinged directly upon military function.

Like deference to women, defense of honor was a major feature of the collective identity of the officer corps. An insult to the officer's person, his regiment, the army as a whole, or the tsar required instantaneous response. General Dragomirov urged that "the slightest hint of an insult by deed [should elicit] immediate reflex retribution with arms."<sup>21</sup> The officer involved decided whether or not his honor had been impugned, but it was best not to take too narrow a view, since a regimental court of honor determined subsequently whether or not an officer had reacted properly and expelled any officer judged indecisive.<sup>22</sup> As a consequence, officers murdered lawyers who were felt to have insulted the army in court proceedings; they beat up or murdered civilians who showed a lack of respect in restaurants or on the street; they set their soldiers upon insolent cabbies—the pretexts for taking revenge were infinite. There were far more incidents of this sort in the tsarist empire than, for example, in Germany, no doubt because Russian civilians were less inclined to share the officers' view of the military's unique honor.<sup>23</sup> Officers were especially sensitive to insults after the Russo-Japanese War and the 1905 revolution—they had much to be sensitive about—and young officers walked the streets with pistols tucked in their pockets, fully prepared to exact immediate revenge. After the 1905 revolution, too, even playing the tsarist anthem in restaurants was inadvisable: scandals occurred because officers felt it their duty to punish civilians who failed to rise or made ironic remarks or gestures.<sup>24</sup>

<sup>20</sup> Only a few generals actually had wives living with them permanently, because there was a rule that wives could not accompany their husbands to the front. B. V. Gerua wrote that General Stakelberg's wife rode with the general to the front line to order withdrawal and that a colonel's wife was seen riding about on horseback encouraging the medics who were collecting wounded soldiers from the battlefield; Gerua to his wife, August 15, 1904 (typed copy), "Pis'ma, 1904-05," BA. I. Taburno, a special correspondent for the archconservative *Novoe vremia*, reported on wives in the medical services; Taburno, *Pravda o voine* (St. Petersburg, 1905), 79-81, 123-24.

<sup>21</sup> As quoted in V. G. Korolenko, *Tragediia generala Kovaleva i navy voennoi sredi* (Moscow, 1906), 21. The same instruction was given to junkers in the Elizavetgrad cavalry school in the fall of 1914; De-Vitt, "Tritsat'piat' let," 11r.

<sup>22</sup> For a discussion of the military regulations pertaining to courts of honor and the somewhat different way in which they operated in practice, see L. Gladkov, "O sudakh chesti," *Voennyi sbornik*, no. 10 (1912): 93-100. Gladkov pointed out that there was some difficulty in interpreting the point at which honor had been impugned and revenge required. Also see Bragin, "Vospominaniia," 1: 107; Buak, "Vospominaniia," 22; Drozd-Boniachevskii, "Poedinok' Kuprina," 1: 183; and Krassnoff, *Double Eagle*, 160-61.

<sup>23</sup> Korolenko, *Tragediia generala Kovaleva*, 1-25. An editorial response to Korolenko appeared in a newspaper article, "Ob etike ofitser'skoi sredi," *Russkii invalid*, October 11, 1905. Also see Kriukov, "Armeiskaia didaktika," 146; Grulev, *Zloby dnia*, 51-52, and *Zapiski*, 213; Birkin, *Osinoe gnezdo*, 240-43; and Pil'skii, "Armiia i obshchestvo," *Mir bozhii*, no. 7 (1906): 210, 213-21.

<sup>24</sup> Littauer, *Russian Hussar*, 93, 98; Birkin, *Osinoe gnezdo*, 18-19; and Ivanko, "Sluzhba v polku," 279-80. One version of the incident that led to the ban on the national anthem relates that an officer murdered a student in a restaurant; John Foster, *Red Russia* (New York, 1907), 4-5.

An apparently extreme but ultimately acceptable instance of the defense of the uniform illustrates the permissible limits of retribution. In 1910 a newspaper in Kazan published in boldfaced type a notice to the effect that citizen N was seeking a ruffian to defend him from officers of the 163d regiment (stationed in Simbirsk). The officers of the 163d chose by lot two of their number to avenge the insult. The two traveled to Kazan, walked into the editorial office, asked for the editor, and murdered him in cold blood. They then returned to their unit and turned themselves over to their commander. They eventually received a pardon from the tsar.<sup>25</sup> That case is in every particular representative. Even in the 1880s, when newspapers were considerably more circumspect than they were in 1910, they occasionally reported incidents that cast a regiment in a bad light. The guards regiments had an established procedure for settling such accounts: a party of officers was to ask the editor to summon the author of the story, and, if the editor refused to name the culprit, the editor himself was to be struck several times with a riding crop.<sup>26</sup> Nor was the Kazan incident unusual in the leniency accorded the murderers. A pamphlet issued to graduates of military schools in the years just before World War I observed that, although officers were answerable to the law if they murdered civilians in defense of the honor of the uniform, "in practice all such cases have been submitted either by the military court or, even before trial, by the military command, to the monarch's mercy, and punishment either has been disallowed or has been significantly reduced."<sup>27</sup> In such circumstances, there was no reason why an officer should risk expulsion from his regiment by being overly liberal in his treatment of civilians.

The tsarist officer's code of honor was rigid and the rules of his decorum refined, but they had—as should now be evident—a remarkably narrow range of application. As one officer observed, should a civilian be so much as discourteous even to a junker (an officer candidate), the insult would be immediately punished. An officer who was insufficiently decisive would without fail be compelled by a court of honor to challenge the civilian to a duel if the civilian were *satisfaktionsfähig*. Yet officer society tolerated embezzlers, grafters, and gigolos.<sup>28</sup> This memoirist did not add—no doubt because it was self-evident—that officer society also suffered in its midst drunks and cardsharps. In fact, an officer could do just about anything without risking expulsion, on the sole condition that he never besmirch the reputation of his unit by permitting his behavior to become known to civilians. Any officer seen drunk on the street, who became known in civilian society as a thief, or whose sexual escapades with socially prominent women became a public scandal would be asked to leave the regiment.<sup>29</sup> What went on within the regiment was another matter.

Tsarist officers lived by firmly established rules that emphasized corporate honor but allowed considerable leeway for drunkenness and theft, to name only

<sup>25</sup> Bitenbinder, "Vospominaniia," chap. 5, p. 6.

<sup>26</sup> Buak, "Vospominaniia," 34-35.

<sup>27</sup> Kul'chitskii, *Sovety ofitseru*, 22-23; and Kriukov, "Armeiskaia didaktika," 145.

<sup>28</sup> Bragin, "Vospominaniia," 2: 107.

<sup>29</sup> Birkin, *Osinoe gnezdo*, 74; Littauer, *Russian Hussar*, 47; Polovtsoff, *Glory and Downfall*, 17-18; Buak, "Vospominaniia," 28-31; Krassnoff, *Double Eagle*, 61-64, 102, 204-05; and Rodzianko, *Tattered Banners*, 34-35.



two of the officer corps' most striking traits. The various social norms were inter-related—drunkenness and gambling with theft; theft with the costly attributes of *politesse*; *politesse* with the central role of wives (as objects of *politesse*) and caste honor; caste honor with social endogeny; social endogeny with the maintenance of drinking customs; and so on. Thus mutually reinforced, the social patterns were deeply imbedded in officer society. None contributed to military efficiency, and some (theft and the role of women, in particular) manifestly subverted efficiency. The officers' social mores, in other words, suggest that their lives did not revolve around their military mission.

There were, of course, more or less substantial deviations from the norm, and local standards were set by the regimental commanders. There were also some slight differences in behavior from one region and one branch of arms to another. (Guards officers had an important role in elite civilian society and had to meet correspondingly higher standards of public decorum.) Yet what stands out is the relative uniformity of social mores in the officer corps. This is an important point, because tsarist officers themselves ordinarily had little sense of unity: the divisions among them were multiple and deep. Nobles and commoners were not equal in status or in prospects for promotion. Guards officers moved in a world apart from line officers. Cavalry, artillery, and engineering officers disdained the infantry and each other. Officers who came from the cadet corps and the military schools (*voennye uchilishcha*) disdained those commissioned out of the plebian junker schools. General staff officers formed a caste within a caste. These divisions were bred in the schools and reinforced by the absence of organizational and even personal contact between the different arms.<sup>30</sup> Yet, despite all of the differences among them, tsarist officers—from the guards cavalry in St. Petersburg to the sappers in the Caucasus to the humblest infantry outpost in Siberia—shared a common pattern of social behavior. Viewed from without, officers were all of a type. The officers' corporate behavioral patterns provided a unity of which they themselves could scarcely conceive.

Had tsarist officers not had a military mission, it would be inappropriate to judge the virtue of their corporate behavior. Yet Nikolai Voronovich's circumstantial memoir on the Horse Grenadiers shows that tsarist officer society functioned poorly even in nonmilitary terms. The Horse Grenadiers formed part of the Guards Corps, but they were garrisoned at Peterhof, some distance from St. Petersburg, and the officers of the regiment were only marginally involved in the social life of the capital. Voronovich, who was commissioned into the regiment a few years before World War I, claimed that the Horse Grenadiers had a long, self-sustaining tradition of exemplary military efficiency. In his fond recollection, the officers did not drink excessively, and in any case drinking did not impair training or performance; all of the officers got along well, and so forth—the officers of the Horse Grenadiers resemble nothing so much as convivial,

<sup>30</sup> Stein, "Der Offizier des russischen Heeres," 374–92; Kenez, "Profile of the Prerevolutionary Officer Corps," 131; Rittikh, *Russkii voennyi byt*, 212–13; M. Galkin, "Ofitserskii vopros," *Voina i mir* (Moscow), no. 7 (1906): 14–15, and "Roza ili bratstvo?," *Voennyi sbornik*, no. 5 (1914): 21–34; Buak, "Vospominaniia," 33–34; Denikin, *Staraiia armia*, 2: 113–24; and Kersnovskii, *Istoriia russkoi armii*, 3: 616–18.

high-spirited, yet hard-working fraternity brothers.<sup>31</sup> But within the space of three years, the regiment's thirty-six officers were involved in a series of incidents that belie Voronovich's characterization. In one two-week period, three officers committed suicide—two for unknown reasons, one because of gambling debts that could not be covered. The epidemic of suicides left Voronovich the only functioning officer in his squadron; the squadron commander, though still among the living, was awaiting transfer to a desk job and absented himself from his duties.<sup>32</sup> Then two of the Horse Grenadiers' well-loved officers fought a duel; both were soon compelled to quit the regiment. Finally, a lieutenant celebrating his twenty-first birthday murdered a soldier left temporarily in charge of the officers' mess because the soldier refused to give him a bottle of champagne on credit; the lieutenant had already reached the two-hundred-ruble maximum of indebtedness to the mess. He was broken to the ranks.<sup>33</sup>

Six of the thirty-six officers of the Horse Grenadiers—by Voronovich's account a happy, industrious group in a crack regiment—fell by the wayside in three years. So high a rate of peacetime attrition is not a mark of social health, especially since that attrition was the result not of accident, biology, or performance ratings but of the social norms central to officer life. Attrition in the Horse Grenadiers in this brief span seems to have been higher than average, but not unusually so. Other memoirs, although they do not provide so complete an account of the life of individual units, tell much the same story.<sup>34</sup> And this is the story told, in different ways, by Kuprin and Krasnov.

MEMOIRS, BELLES-LETTRES, AND CONTEMPORARY CRITICAL LITERATURE depict not just the social demoralization of officer life but also—and in equal measure—indifferent performance of military duties. The literature mentions, even more frequently than drinking, the incompetence of command personnel and the absence of a military tone among all other officers. Contemporaries claimed that the vast majority were indifferent to even the most basic elements of tactics. Tactical gaming repeated the same problems from standard texts year after year, and “tactical discussions” in winter months ordinarily did not go beyond the study of regulations.<sup>35</sup> Officers demanded as little of their men as they did of

<sup>31</sup> Voronovich, *Vechernyi zvon*, 148, 150, 161.

<sup>32</sup> *Ibid.*, 159. Krasnov remarked that suicides by officers “formed part of army life, the measures to be taken were laid down in the regulations”; Krassnoff, *Double Eagle*, 157. Also see Denikin, *Staraiia armiia*, 1: 94, 2: 84–86; and Rodzianko, *Tattered Banners*, 109. The impressionistic evidence on suicides among tsarist officers indicates that the rate was abnormally high, which suggests a genuine problem of demoralization.

<sup>33</sup> Voronovich, *Vechernyi zvon*, 177–79, 181–86.

<sup>34</sup> Between 1902 and 1914 the number of officers in His Majesty's Cuirassiers fluctuated around thirty-seven. There was one suicide each in 1907 and 1908, and in 1908 another officer had to be confined to an insane asylum. Between 1902 and 1911 there was an annual average of two and a half retirements or transfers (excluding the departure of two colonels to command line regiments); Devlet-Kil'deev, *Kirasiry Ego Velichestva*, 17–26. The reasons for this attrition are not indicated, although it appears not uncommon for officers to have been compelled to quit the regiment for unworthy conduct (for example, failing to pay debts); N. A. Petrovskii, “Vospominaniia,” *ibid.*, 34.

<sup>35</sup> B. V. Gerua, *Vospominaniia*, 67, 83, 97; Smirnov, “Ofitserskii vopros,” 146; F. Gershel'man, “Takticheskie zaniatiia v voiskakh,” *Voennyi sbornik*, no. 12 (1906): 71–72, 79; E. I. Martynov, *Iz pechal'nogo opyta russko-iaponskoi voiny* (St. Petersburg, 1906), 40–45; Denikin, *Staraiia armiia*, 1: 23–27; Bragin, “Vospominaniia,” 1: 32–34; Nikolai Butovskii, *Ocherki sovremennogo ofitserskogo byta* (St. Petersburg, 1899), 11–13, 31–32; Krasnov, *Nakanune*

themselves. Training focused on the manual of arms and close-order drill; the skirmish line and use of cover were rarely practiced. Soldiers never learned more than was taught in the first four months after induction, because veterans were subjected to the same drill as draftees. Training so unchallenging and so routinized could be, and was, entrusted to the sergeants-major; officers merely dropped in now and again to see how things were going.<sup>36</sup>

The fighting quality of the army was tested in the spring review and summer maneuvers, but both stressed the outward appearance of military polish—the ability to hold formation and keep proper intervals—whether soldiers were marching on the drill field or charging up a hill. Every year the army conducted the same exercises over familiar terrain, with distances and movements lovingly calibrated in advance. The prototypical scene at summer encampment involved a regiment preparing for the arrival of the high command by practicing, for the hundredth time, an attack on a copse. Soldiers learned to rise and fall in unison and to converge on the object uniformly. Sergeants carefully marked out the spots for each jump and, in order to ensure that even the dullest would be able to hold perfectly to the converging fan formation, scuffed out tracks for each soldier to follow. Then the colonel issued his final instructions: “Although units in line of battle should charge with a ‘hurrah’ individually as they approach the enemy, it is my opinion that at inspection this is unattractive, so I sincerely request that all companies take their cue from the 2d [company] and charge with a ‘hurrah’ simultaneously.”<sup>37</sup> When a regiment broke camp for maneuvers, it took along the complete inventory of the officers’ mess. To accommodate the train and to provide attendants with the time required to pitch an elaborate camp, nightly bivouacs were fixed well in advance and converted into a rendezvous toward which the train proceeded directly, the troops circuitously. Maneuvers were reduced to little more than extended marches that culminated, for the officers, in picnics.<sup>38</sup>

voiny, 19–20; Littauer, *Russian Hussar*, 107; Grulev, *Zloby dnia*, 57–58, 146–48; Zaionchkovskii, *Samoderzhavie i russkaia armia*, 175–77; Zalesskii, *Vozmezdzie*, 63, 65; and Staryi, “Kavaleriiskie mysli i nabroski,” *Voennyi sbornik*, no. 6 (1906): 127.

<sup>36</sup> Rittikh, *Russkii voennyi byt*, 82–83, 143–45; Nikolai Butovskii, *Novoe napravlenie v smotrovnykh trebovaniakh* (St. Petersburg, 1893), 12–13, 19–20; M. Zenchenko, *Obuchenie i vospitanie soldata: Soobshchenie* (St. Petersburg, 1902), 59, 63; Rezhepo, *Neskol'ko myslei*, 6; M. Galkin, *Novyi put' sovremennogo ofitsera* (Moscow, 1906), 51, 59; Denikin, *Staraia armia*, 2: 180–83; Zaionchkovskii, *Samoderzhavie i russkaia armia*, 253–56; Ladyzhenskii, “O mirnoi podgotovke kavalerii k voine i o ‘Nastavlenii dlia vedeniia zaniatii v kavalerii,’” *Voennyi sbornik*, no. 2 (1906): 96–98, and no. 3 (1906): 115, and “O komandnom sostave armii i sisteme ego podbore,” *Voennyi sbornik*, no. 10 (1906): 168; K. Lazarevich, “Sovremennye tendentsii v strelkovom dele na zapade,” *Voennyi sbornik*, no. 3 (1906): 90; Grulev, *Zapiski*, 92, 98; Littauer, *Russian Hussar*, 110–11; and *Voennaia reforma* (St. Petersburg, 1906), 59.

<sup>37</sup> Butovskii, *Novoe napravlenie*, 3–6. Butovskii claimed that this was typical of the “bad old days,” but such days had not yet passed. For examples of this continuity, see Zalesskii, *Vozmezdzie*, 74, 85; Fedor Stepun, *Byushee i nesyusheesia*, 1 (New York, 1956): 81–82; Denikin, *Staraia armia*, 1: 94; Ladyzhenskii, “O miroi podgotovke kavalerii,” 3: 115–17; and Petrovskii, “Vospominaniia,” 65–67.

<sup>38</sup> Bivouac life is described by Petrovskii, “Vospominaniia,” 65–67; and V. S. Zholtenko, *Prostodushnye: Povest'* (Harbin [1938]), 86–88. In contrast to German practice, the Russian army did not include trains in its war games calculations; A. Radus-Zenkovich, “Zametki o germanskikh voisk po nabliudenii na podvizhnykh sborakh odnogo germanskogo korpusa v sentiabre 1906 goda,” *Voennyi sbornik*, no. 2 (1907): 139–41. Maneuvers, of course, repeated all of the faults of encampment exercises; see, for example, Staryi, “Kavaleriiskie mysli,” 134–36; Ladyzhenskii, “O komandnom sostave armii,” 141, 145; Krassnoff, *Double Eagle*, 94–95; M. Kvetsinskii, “Tekhnika organizatsii manevra,” *Voennyi sbornik*, no. 1 (1914): 27–32, and no. 2 (1914): 11–16; Zaionchkovskii, *Samoderzhavie i russkaia armia*, 253–56, 279–80; and Kersnovskii, *Istoriia russkoi armii*, 3: 521–22.

This routine of training and exercises was both pointless and monotonous, and it quite understandably bored line officers to stupefaction. Boredom during duty hours in turn bred the vacuity and distemper that marked officer society: as a Colonel Ladyzhenskii noted, "Under such circumstances all that can develop is moral languor and a desire to serve anywhere but in the line."<sup>39</sup> Like the social customs it complemented, the prevailing service routine was normative. Officers who sought to improve their skills met with the suspicion, scorn, or simple incomprehension of their commanders and peers alike. To display initiative or zeal was the mark of a "restless" or "revolutionary" officer, and either interpretation was likely to impede promotion.<sup>40</sup> Observers frequently noted the want of initiative of any sort at all levels of command. An evaluation of the tsarist army by the German general staff in 1913 concluded that Russian commanders reacted slowly to favorable operational situations and that German generals could indulge in tactical maneuvers against Russian troops that they could not risk against any other army.<sup>41</sup> Some traced tsarist officers' lack of efficiency and initiative to uninspiring instruction in the military schools, but most newly commissioned officers arrived in their units with a natural zeal to prove themselves. Once in the regiments, however, as General M. Grulev observed in 1911, they "immediately run up against deeply rooted views and customs that have raised *pro forma* execution of duties, idleness, and sloth to a cult, while exertion and a serious attitude to work provoke scowls from the commander and irony from comrades."<sup>42</sup> Young officers quickly mastered the art of inefficiency.

To say that disengagement from military duties was learned behavior does not answer more fundamental questions: Why, in an institution whose principal function ought to have been preparation for war, were military skills so long devalued? How could the routine survive even shocking failure on the battlefield? The answers are found neither in the system of military pedagogy nor in any absence of wit. Bureaucratization, centralization of control, and the very duration of these habits all contributed. But the logistics of the tsarist army, in particular the peacetime logistics of the regiment, made this inertia particularly difficult to shake. The tsarist regiment's internal economy was central to its functioning, and in consequence economic concerns displaced all others. The system by which regiments were supplied had changed little since the eighteenth century: central institutions provided weapons and some of the raw materials for food and uniforms; the regiment itself produced most, and procured a good part, of what it consumed.<sup>43</sup> In the early twentieth century, the commis-

<sup>39</sup> Ladyzhenskii, "O mirnoi podgotovke kavalerii," 3: 115. Also see Zalesskii, *Vozmezhdie*, 108-10; and Grulev, *Zapiski*, 119, 123-29.

<sup>40</sup> Rittikh, *Russkii voennii byt*, 159-60; Grulev, *Zloby dnia*, 143-45, 287-91; Bitenbinder, "Vospominaniia," chap. 5 p. 5; Yakhontoff, *Over the Divide*, 31-33, 73-74; N. N. Golovin, *Mysli ob ustroistve budushchei russiiskoi vooruzhennoi sily* (3d edn., Paris, 1930), 42; Birkin, *Osinoe gnezdo*, 89-90; A. Gerua, *Posle voiny*, 43-44; Martynov, *Iz pechal'nogo opyta*, 34-35, 42-43; and K. Oberuchev, "Voennii podbor," *Russkii vestnik*, no. 4 (1911): 156-59.

<sup>41</sup> Golovin, *Mysli ob ustroistve*, 34.

<sup>42</sup> Grulev, *Zloby dnia*, 148. Also see *ibid.*, 137-38, 287-91. Other observations in the same vein can be found in Rittikh, *Russkii voennii byt*, 46; Butovskii, *Sbornik poslednykh statei*, 94-95; Saryi, "Kavaleriiskie mysli," 129; and Ladyzhenskii, "O mirnoi podgotovke kavalerii," 3: 115.

<sup>43</sup> Primitive transport and the size of the empire had originally dictated a system of regimental self-sufficiency. This system survived to the early twentieth century with only minor changes because the alternative,

sariat provided either flour or grain (which regiments turned into bread) and money to purchase other foodstuffs and all fodder on the market. Many regiments had, in addition, their own vegetable gardens, their own cattle, and, until the 1890s in outlying regions, their own sizable agricultural estates.<sup>44</sup> The commissariat provided leather and cloth (or money to purchase them on the market) from which the regiment manufactured its own uniforms. There were up to two hundred full-time cobblers and tailors in a regiment (out of a peacetime complement of seventeen hundred men), and in 1907 one hundred and fifty thousand soldiers—12 percent of all enlisted personnel—spent their duty hours tailoring. If a regiment had blankets, coats, and cooking and eating utensils (and not all regiments had blankets or coats), they had been acquired at the regiment's own expense, because the commissariat allocated neither money nor material for them. The regiment had to produce a wide range of other necessities as well. In a typical regiment roughly 50 percent of all soldiers who had completed basic training were employed in production of one sort or another during the winter months. A contemporary description of the tsarist regiment as a department store, and the commander as a quartermaster-sergeant, does not appear exaggerated.<sup>45</sup>

The tsarist regiment survived economically by saving what money it could from centrally budgeted items. These savings formed the regiment's working capital. So vital to the functioning of the regiment was its capital reserve that, when an officer accepted a command (or in the rare case when he was offered a choice of regiments), his first order of business was to ascertain the state of the regiment's capital.<sup>46</sup> Capital was in fact never adequate for maintenance and supply, but the regiment did have an abundance of one resource: manpower. Not only did soldiers work the regiment's own land and capital stocks, but they were also sent out for a month or so every autumn to earn money in the civilian economy. During autumn months the regiment virtually ceased to exist as a

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though practicable, was more expensive; A. Gerua, *Posle voiny*, 66. On the Russian army in the eighteenth century, see *Stoletie voennogo ministerstva, 1802–1902*, volume 5: *Glavnoe intendantskoe upravlenie: Istoricheskii ocherk*, part 1: *Vvedenie i tsarstvovanie Imperatora Aleksandra I* (St. Petersburg, 1903), 1–129. On supply in the early nineteenth century, see John S. Curtiss, *The Russian Army under Nicholas I, 1825–1855* (Durham, N.C., 1965), 211–20. There was some interest in changing the system in the 1860s and 1870s, but the only substantial change was the introduction of a better, but much more complicated, system of accounting; Zaionchovskii, *Voennye reformy*, 48, 60, 124–25. Also see L. G. Beskrovnyi, *Russkaia armia i flot v XIX veke* (Moscow, 1975), 453–86. Beskrovnyi has discussed, quite inadequately, the system of supply for the entire nineteenth century; he has mistakenly asserted that production within the regiments ended in 1874.

<sup>44</sup> Regimental and company gardens were provided for in the regulations, and guards regiments especially followed this practice; *Polozhenie o khoziaistve v rote* (St. Petersburg, 1878), 40–41. Also see F. A. Maksheev, *Voennoe khoziaistvo v miroe vremia v armiakh: Russkoi, germanskoi, avstriiskoi i frantsuzskoi (Sraunitel'nyi ocherk sovremennogo ustroistva ego)* (St. Petersburg, 1904), 125–41, 332–34; Z. Kononenko, "Novoe polozhenie o khoziaistve v polku," *Voennyi sbornik*, no. 12 (1912): 78; Fon Dreier, *Na Zakate imperii*, 18; A. P. Voznesenskii, "O voennom khoziaistve," *Obshchestvo revnitel'no voennykh znaniu*, no. 4 (1906): 109; Grulev, *Zloby dnia*, 229; and N. A. Baikov, *Taiga shumit* (Harbin, n.d.), 100.

<sup>45</sup> Maksheev, *Voennoe khoziaistvo*, 141–42, 402, 404, 662; A. M. Volgin, *Ob armii* (St. Petersburg, 1908), 114–16; A. Gerua, *Posle voiny*, 48–51, 65; Voznesenskii, "O voennom khoziaistve," 111, 118; R. Sul'menev, "Nedostatki nashego voiskovogo khoziaistva," *Voennyi sbornik*, no. 5 (1906): 194–96; and Denikin, *Staraiia armia*, 2: 178–80.

<sup>46</sup> Krasnov, *Na rubezhe Kitaia*, 113; Sul'menev, "Nedostatki nashego voiskovogo khoziaistva," 197–201; and Maksheev, *Voennoe khoziaistvo*, 334, 405, 670, 673.



military unit, and even during winter training months it was unusual to have more than a dozen men per company at drill (other than the current crop of draftees, who drilled separately). Under such circumstances, even the most zealous officer found it difficult to train his men, and the circumstances discouraged zeal.<sup>47</sup>

The structure of the regimental economy distracted officers as well as men from military duties: a very high proportion of officer time and manpower was necessarily devoted to managing production and procurement. The colonel was responsible for his regiment's economic well-being and also for seeing that every expenditure (even if funded by the regiment) conformed to regulations. Every item of income and expenditure, no matter how small, was entered in at least five separate accounts, and each entry—even bookkeeping transfers from one account to another—required signed authorization from the commander (he signed over one hundred every day) and had to be communicated in the regimental order of the day.<sup>48</sup> Because he bore full responsibility for every misappropriation, whether genuine or merely formal, it was not uncommon for commanders, when surrendering their regiments, to leave thousands of rubles on deposit to cover any expenditures that government auditors might eventually deem unauthorized. (At the same time, so intricate was the web of regulations that one could draw out an inquiry for years by citing obscure or even irrelevant articles, until the tsar issued one of his periodic blanket amnesties forgiving the hapless colonels their official debts.)<sup>49</sup>

The colonel did not bear this burden alone. No fewer than nine of the sixty or so officers in a line infantry regiment were assigned to bookkeeping and the management of procurement and production (some combined this with duties in the line).<sup>50</sup> Prior to the 1880s, companies had functioned as autonomous economic units, because soldiers were quartered on the civilian population. Even after the centralization of baking and most procurement at the regimental level (as regular barracks were built), company bookkeeping continued as before. The company commander kept track of "company" expenditures over which he had no control. Production and procurement of boots and underwear were still car-

<sup>47</sup> All of the sources given in note 45 mention the detrimental effect of the regiment's economic structure on training; also see Denikin, *Staraia armia*, 2: 178-80; Bushnell, "Peasants in Uniform," 567-69; Ladyzhenskii, "O miroi podgotovke kavalerii," 2: 99; Petrovskii, "Vospominaniia," 77; Vs. Sakharov, "Mysli po sovremennym voprosam," *Voennyi sbornik*, no. 6 (1907): 64-67; and *Polozhenie o khoziaistve vrote*, 44-48, 71-75. Guards regiments did not differ in this respect from line regiments.

<sup>48</sup> The commander's responsibilities for the regimental economy were enumerated in *Polozhenie ob upravlenii polkom: S prilozheniem I i II razdela polozheniia ob upravlenii khoziaistvom v otdel'nykh chastiakh voisk* (St. Petersburg, 1882), 48-50. On the consequences of these obligations, see Kononenko, "Novoe polozhenie o khoziaistve v polku," 78, 83-84; A. Gerua, *Posle voyny*, 54; Maksheev, *Voennoe khoziaistvo*, 667-70; and G. Iakubovskii, "Po voprosu ob uproshtchenii i sokrashchenii voiskovoi denezhnoi otchetnosti," *Voennyi sbornik*, no. 2 (1910): 122, 130.

<sup>49</sup> *Polozhenie ob upravlenii polkom*, 49; Voznesenskii, "O voennom khoziaistve," 112; Butovskii, *Stat'i na sovremennye temy*, 42; Zambrzhitskii, "Ocherki bylogo," 11; and Maksheev, *Voennoe khoziaistvo*, 79-83.

<sup>50</sup> The obligations of the five officers formally charged by the regulations with full-time economic management were spelled out in *Polozhenie ob upravlenii polkom*, 50-54. Also see Maksheev, *Voennoe khoziaistvo*, 655-56; and Voznesenskii, "O voennom khoziaistve," 113. At full strength an infantry regiment in peacetime had seventy officers, but there was never a full complement.

ried out at company level. Should any officer somehow have escaped direct involvement in management, he was placed on one of the many committees that every month verified the books kept by other officers.<sup>51</sup>

The operation of the regimental economy was obviously detrimental to military efficiency: the amount of management and bookkeeping demanded of officers left little time for training soldiers. "Exemplary" commanders were those who spent their days in the office composing lengthy orders and reports for higher staffs, while commanders who supervised training failed to complete their paperwork and so earned the displeasure of the staffs. Observed one general staff colonel, "Life itself leads the commanders of regiments to pass their service in the office."<sup>52</sup> Although staff-grade officers might under any circumstances have been absorbed in paperwork, in the tsarist army even the captains who were directly responsible for training spent fully half of their time tending nine separate account books (with three extra in the cavalry to cover fodder and shoes), composing the orders and other papers that documented the circulation of goods and cash, and, most assuredly, seeing that company property was in good order and that a cash surplus was built up by economizing on budgeted expenditures. After the morning labor of "signing records, reports, urgent and not so urgent dispatches, armor lists, soldiers' account books, provisions lists, authorizations," and so on, after reading the pamphlet-length regimental order of the day, and after tasting as per custom the soldiers' noontime meal, the captain would "hasten to the mess, chalk a cue, and play a round or two of billiards before supper, to stretch, so to speak, and then to sit for an animated meal" (with "a little vodka, herring, appetizers. . .").<sup>53</sup> Spending the afternoon in the mess was as natural as attempting to make up for lost time in the barracks, since most soldiers would in any case be laboring for the benefit of the regimental economy.

Much of the red tape that swaddled the regiment's economic activity had been cultivated to prevent abuses; the tsarist officer's urge to avail himself of opportunities for financial gain was well known. Although the colossal abuses of the past no longer occurred, the potential for petty corruption was proportional to the density of the often contradictory rules and regulations. The bookkeeping

<sup>51</sup> Voznesenskii, "O voennom khoziaistve," 114-18, 122; Kononenko, "Novoe polozhenie o khoziaistve v polku," 79, 85-86; and Maksheev, *Voennoe khoziaistvo*, 84, 334, 402, 678-86.

<sup>52</sup> F. P. Rerberg, "Polevoe upravlenie voisk po opytu voyny," *Voennyi sbornik*, no. 3 (1906): 168.

<sup>53</sup> This description of the routine of a captain in the Sixth Sapper battalion in Kiev in 1906 is provided by Zambrzhitskii, "Sapernyi bunt" [13]. Zambrzhitskii asserted that this captain was typical of sapper captains in Kiev prior to the Russo-Japanese War, but he claimed that most officers in the sixth battalion (which had fought in Manchuria) had abandoned the old order. This captain was indeed typical of the period prior to 1905 and also, to a great degree, of the period thereafter, both in the line and in the guards. On captains' behavior and the immense amount of their time consumed by bookkeeping, see Svechin, *Zapiski*, 56-57; Polovtsoff, *Glory and Downfall*, 123; Butovskii, *Stat'i na sovremennye temy*, 30-31; B. v. Rechenberg-Linten, *Russische Soldaten und Offiziere aus der Zarenzeit: Nach Selbsterlebnissen in einer russischen Garnison* (Bern, 1924), 13-22; Rittikh, *Russkii voennyi byt*, 75; N. D. Butovskii, *Nashi soldaty: Tipy mirnogo i voennogo vremeni* (St. Petersburg, 1893), 83; Grulev, *Zloby dnia*, 9-15; Galkin, *Novyi put'*, 49-50; Staryi, "Kavaleriiskie mysli," 132; and Ladyzhenskii, "O mirnoi podgotovke kavalerii," 2: 102. The details of the company commander's economic obligations and sample pages from the books he had to keep are provided in the 214(!) pages of *Polozhenie o khoziaistve v rote*. On the order of the day, see *Polozhenie ob upravlenii polkom*, 35-37.

system alone was so complicated that auditing the books required the compilation of a separate set of accounts, and it could take years to track down genuine peculation. Indeed, formally illegal disbursements were required for the regiment's own good simply because the commissariat provided too little money even for the most necessary expenses. As one officer put it, "Oats turn into coats, a carriage springs from firewood, prayers are paid for in cabbage."<sup>54</sup> The commander picked the best officers for management, although "best" meant either those with expertise in bookkeeping and knowledge of obscure regulations that justified questionable expenditures or those with a proven ability to steal. An emigré officer observed, after describing a particularly thievish officer, "How many such gentlemen as he went unpunished! The command sometimes even singled out such inventive types as capable and industrious officers."<sup>55</sup>

The bookkeeping sleight-of-hand that was a necessity for the smooth functioning of the unit provided more than adequate training for the many varieties of theft and misappropriation in which officers indulged, but that was probably the least important way in which the regiment's economy shaped the mentality of the tsarist officer corps. Since officers of captain's rank and above had no time to drill their men, it is scarcely surprising that they should have identified the essence of military service with work in the offices rather than with drill in the field or barracks. More interestingly, the same held true for lieutenants. Only junior officers, unless assigned to the regimental staff because they displayed a precocious talent for keeping books, were spared the all-consuming paperwork that was the lifeblood of the regiment. Yet they had no regularly assigned duties until 1908, when an order was finally issued that junior officers be given responsibility for some aspect of training. So strong was the association between officers and desk work that many regimental and company commanders really did not know what to do with officers who had not risen to desk grade.<sup>56</sup> Accordingly, junior officers learned by example that an officer's proper place was in an office, and that was where they gravitated. (The flow of paperwork was thereby impeded, so in some units signs were posted warning junior officers to keep out of the offices unless they had business there.) The well-known rule was that lieutenants had to put in no more time in the barracks than did their captains—which is to say, not much. When coerced into the barracks, lieutenants idled on the sidelines, gossiping and watching the clock. Work with soldiers was thought a great bore.<sup>57</sup>

<sup>54</sup> Kononenko, "Novoe polozhenie o khoziaistve v polku," 85; Voznesenskii, "O voennom khoziaistve," 111, 122; A. Gerua, *Posle voyny*, 54; Grulev, *Zloby dnia*, 239; Butovskii, *Stat'i na sovremennye temy*, 35; and Iakubovskii, "Po voprosu ob uproschenii i sokraschenii voiskovoi denezhnoi otchetnosti," 133.

<sup>55</sup> Bragin, "Vospominaniia," 2: 67; also see A. Gerua, *Posle voyny*, 47-49; and Butovskii, *Stat'i na sovremennye temy*, 6-15, and *Sbornik poslednykh statei*, 69, 120-22.

<sup>56</sup> Mstislav Levitskii, *Vospitanie soldata* (St. Petersburg, 1911), 45; Butovskii, *Sbornik poslednykh statei*, 115; Gal-kin, *Novyi put'*, 136; Birkin, *Osinoe gnezdo*, 74; Smirnov, "Ofiterskii vopros," 151; Rezhepo, *Neskol'ko myslei*, 8; Staryi, "Kavaleriiskie mysli," 129; V. M. Dragomirov, "Podgotovka russkoi armii k voine," *Voennyi sbornik* (Belgrade), 5 (1924): 199; Zalesskii, *Vozmezhdie*, 62-63; Buak, "Vospominaniia," 14; and Ladyzhenskii, "O mirnoi podgotovke kavalerii," 2: 102.

<sup>57</sup> Butovskii, *Stat'i na sovremennye temy*, 36, 39-41, and *Sbornik poslednykh statei*, 94-95; Gal-kin, *Novyi put'*, 49-50; Zenchenko, *Obuchenie*, 59-61; Grulev, *Zapiski*, 94, and *Zloby dnia*, 54, 57-58, 148; *Voennaia reforma*, 59; and A. V. Evdokimov, "Bol'shoe delo molodogo ofitsera," *Voennyi sbornik*, no. 10 (1909): 107.

If the regiment's economy pinned officers to their desks, it also nourished a proprietary spirit. Officers of all ranks viewed the regiment as an estate, the resources of which could be used for their personal needs and pleasures—from building summer cottages at encampments to outfitting apartments. So deeply ingrained was this attitude that, when a fundamental reform of the regimental economy was proposed after the Russo-Japanese War, officers resisted because they could not imagine how to do without the services of the regimental workshops. This outlook also shaped the officers' relations with their men, who were viewed as so much free labor (serfs, as General Dragomirov forthrightly remarked) at the officers' disposal.<sup>58</sup> Complementary to the proprietary instinct was a deep-seated (and perhaps unconscious) pacific spirit. Despite constant colonial warfare and a major war every generation, the tsarist officer corps did not, on the whole, think of war as a likely eventuality.<sup>59</sup> When their primary attention was devoted to economic management, they could not think gladly about the ruin war would bring to the regiment's capital. More fundamentally, it was impossible for officers to spend the bulk of their time ignoring tactics and training and at the same time to conceive of war as a real possibility. Ambitious young officers may have dreamed of martial glory, but the regimental routine nurtured an entirely different set of values. The system taught, wrote one officer, "exact knowledge of procedure, the ability to manage the [regimental] economy—in a word, the virtues of a peaceful citizen and diligent proprietor."<sup>60</sup>

These values were so deeply imbedded in the pattern of the tsarist officer's life and work that to change them would have required the exertion of enormous pressure from the top of the military hierarchy. But of course the generals—and the system of promotion of which they were a product and which they controlled—reinforced those values. The principal mechanism for rating the performance of a regiment and its officers was the annual inspection by the division commander, and he was interested chiefly in the material externals: Were the barracks well kept? Were stores in order? Had capital reserves been accumulated? The most important point on a colonel's fitness report had to do with economic management: a rating as "good proprietor" was the key to advancement. All that was demanded in the realm of combat efficiency was an absence of obvious blemishes. Similarly, colonels might lose their regiments for bad management, not for military incompetence.<sup>61</sup> The genuine prominence of economic management in the regiment's functions ordained this priority, but the pettiness of the high command's management mentality was at times striking. A colonel

<sup>58</sup> Grulev, *Zloby dnia*, 151, 228–29, 237–38, 269–70; Butovskii, *Stat'i na sovremennye temy*, 62, 64; Zalesskii, *Vozmezdie*, 62–64; Rezhepo, *Neskol'ko myslei*, 5–6; and Bushnell, "Peasants in Uniform," 568–69.

<sup>59</sup> Petr Pil'skii, "Armii i obshchestvo," *Mir bozhii*, no. 6 (1906): 45; Krassnoff, *Double Eagle*, 245; and Zalesskii, *Vozmezdie*, 62.

<sup>60</sup> Rezhepo, *Neskol'ko myslei*, 2.

<sup>61</sup> Butovskii, *Stat'i na sovremennye temy*, 6–15, 31, 44; A. Gerua, *Posle voiny*, 49; Voznesenskii, "O voennom khoziaistve," 112; Sul'menev, "Nedostatki nashogo voiskovogo khoziaistva," 187–91, 197–98; and Kernovskii, *Istoriia russkoi armii*, 3: 523. In the ordinary course of service, promotion for nonfavored groups was based solely on seniority. For a detailed critique of the efficiency rating system, see Ladyzhenskii, "O komandnom sostave," *Voennyi sbornik*, no. 9 (1906): 129–48, and no. 10 (1906): 157–72.

might receive a bad rating because uniforms were not stored in the regulation manner ("tied in bundles of ten, seals on top; boots to hang in clusters"). According to a critic writing in 1907, "Not so long ago, a corps commander who had barely served in the line but had a fine grasp of paperwork began to verify company books and issued reprimands over trifles, such as that threads were not entered in the proper column in the workshop account. Immediately thereafter, it was required that company reports be sent monthly to the division staff, and even the general staff took part in auditing them."<sup>62</sup> Behavior such as this—and there are other examples—lends credence to the contemporary truism that the higher the rank, the less able the officer. It was not so much that promotion selected mediocrity as that promotion selected officers who truly believed that military efficiency lay in the proper accounting of threads and company monies.

The other truism of the day was that the surest means to promotion was to obtain a staff job. There were many for the taking. Formally, in 1908 there were 37,302 officers in the line and 7,478 officers on staffs, but in practice between 10,000 and 12,000 officers counted in the line were either full-time managers on the regimental level, had been detailed to higher staffs or other administrative departments, or were studying in the higher military schools that guaranteed them staff positions. In reality, therefore, there were about 19,000 staff officers to 25,000 line officers. This extraordinary swelling of staffs offered ample opportunity to quit the line and clearly had a negative impact on the level of training of both officers and men. The disproportionate number of staff officers is indicative of the hierarchy of tsarist military priorities, and those priorities are reflected in the more rapid advancement of staff officers. The median length of service for line officers who made colonel was twenty-eight years, but very few line officers achieved that grade. The median seniority for staff officers who made colonel was twenty-five years, and almost all reached that level.<sup>63</sup>

Although all officers may have aspired to staff positions and a chance for upward mobility, they did not all have equal opportunity. Probably the single most important division within the tsarist officer corps was between those who were destined to be promoted above captain and those who were not and knew it. There were two overlapping groups of officers who had far and away the best prospects for promotion: guards officers and general staff officers (GSOs). In 1912, when guards officers accounted for only 4 percent of the entire officer corps, 61 percent of the full generals and 54 percent of the lieutenant generals

<sup>62</sup> Butovskii, *Stat'i na sovremennye temy*, 38, 82. For descriptions of identical behavior, see Fon Dreier, *Na zakate imperii*, 111; and Zaleskii, *Vozmezdie*, 129-30.

<sup>63</sup> Rezhepo, *Neskol'ko myslei*, 4-5, 22-24. Rezhepo did not claim to be referring only to general staff officers, so his figures on rank and seniority appear to embrace both GSOs and non-GSOs who held regular staff positions. In the GSO class of 1899, 40 percent had become colonels after fifteen years of service, 80 percent after seventeen years of service, and all by twenty-one years of service; A. A. Zaitsov, *Shuzhba General'nogo Shtaba* (New York, 1961), 17. Working with the list of generals for 1907, K. M. Oberuchev identified corps commanders who had neither guards nor GSO backgrounds and whose rise can be attributed to the fact that they spent most of their service in administrative positions; Oberuchev, *Nashi voennye vozhdii* (Moscow, 1909), 18-19. Since there were 8,448 colonels in 1912 and 7,478 officers on staffs in 1908, it is theoretically possible that staff (but non-GSO) officers could have accounted for the bulk of colonels; Kenez, "Profile of the Prerevolutionary Officer Corps," 132; and Rezhepo, *Neskol'ko myslei*, 22-24.



had served in the guards. In the same year, when only 2 percent of all officers were on the general staff, 62 percent of the corps commanders, 68 percent of the infantry division commanders, and 77 percent of the cavalry division commanders were GSOs.<sup>64</sup> The special advantages of the guards were due almost entirely to their social background: they were overwhelmingly noble, overwhelmingly graduates of the exclusive military schools in the capital, and on both counts well connected and specially favored—they had patrons and protectors in high places. Furthermore, guards officers on transfer to a line unit were promoted one grade (two grades until 1884), so that it was relatively easy for those with the inclination to command a line regiment.<sup>65</sup>

General staff officers, in contrast, were, to use Peter Kenez's phrase, "an elite of meritocracy"; any officer who could pass the battery of stiff entrance exams could enter the General Staff Academy. Everyone recognized that GSOs were the educational elite of the tsarist army, and their rise through the ranks was smooth and rapid.<sup>66</sup> If they were the most learned, however, it is also true that those who rose to command rank were precisely the officers chiefly responsible for the tsarist empire's military failures, most notably during the Russo-Japanese War and World War I, when GSOs had a virtual monopoly on high command.<sup>67</sup> Nothing in their career patterns had prepared general staff officers to command large formations. They spent their time overwhelmingly in office administration—where their skills in composing orders were truly in demand—and considered their few required years in the line as a springboard for promotion back into a staff. Although their lack of experience in the line was frequently held against them, the real problem was that the GSOs' routine administrative work had absolutely nothing to do with the proper functions of a general staff officer. As one contemporary analyst observed, however, it was pre-

<sup>64</sup> Kenez, "Profile of the Prerevolutionary Officer Corps," 137; and Denikin, *Staraiia armia*, 1: 13. Oberuchev provided data on guards and GSO background for all generals as of 1907, but he counted as guards officers only those who had not become GSOs. As of 1907, 70 percent of all military district commanders, 53 percent of all corps commanders, 51.7 percent of all infantry division commanders, and 36.4 percent of all cavalry division commanders were GSOs; Oberuchev, *Nashi voennye vozhdii*, 52.

<sup>65</sup> Volgin, *Ob armii*, 122; Krasnov, *Na rubezhe Kitaia*, 7–13; Kenez, "Profile of the Prerevolutionary Officer Corps," 133–34, 136–37, 140–41; Stein, "Der Offizier des russischen Heeres," 377–79; Martynov, *Iz pechal'nogo opyta*, 33–36; K. M. Oberuchev, "Voennyi podbor," *Vestnik evropy*, no. 4 (1911): 165; and "Pravda li eto," *Varshavskii voennyi zhurnal*, no. 6 (1902): 611. In the early 1900s, both colonels and two of four squadron commanders of Her Majesty's Cuirassiers transferred to the line and received command of line regiments (guards regiments were commanded by major generals); Svechin, *Zapiski*, 33.

<sup>66</sup> Kenez, "Profile of the Prerevolutionary Officer Corps," 138; A. I. Denikin, *Put' russkogo ofitsera* (New York, 1953), 89–90; B. V. Gerua, *Vospominaniia*, 149; Martynov, *Iz pechal'nogo opyta*, 36; Grulev, *Zloby dnia*, 39; and Zaionchkovskii, *Samoderzhavie i russkaia armia*, 190–93.

<sup>67</sup> In 1905 general staff officers received considerable criticism in the press. For the reaction of a young GSO, who attempted to distinguish between command-rank GSOs and the younger generation, see B. Gerua to his wife, April 22, 1905, "Pis'ma, 1904–05." It was the opinion of General A. A. Bilderling, who had commanded a corps in the Russo-Japanese War, that GSOs actually doing staff work performed well, although they were not technologically up-to-date and were too frequently assigned to administrative duties in the rear where their expertise was not required; Bilderling, "Prokhozhdienie sluzhby i podgotovka ofitserov General'nogo Shtaba po opytu minuvshiei voiny," *Voennyi sbornik*, no. 6, (1906): 181–88. Bilderling did not comment on the performance of command-rank GSOs. For the opinion of a line officer who believed that GSOs had so compromised themselves during the Russo-Japanese War that they ought to have been done away with, see Birkin, *Osinoe gnezdo*, 37–39. On the dominance of GSOs in the high command during World War I, see Matitiahu Mayzel, "The Formation of the Russian General Staff, 1880–1917," *Cahiers du monde russe et sovietique*, 16 (1975): 116–17.

cisely this routine staff work that was thought to be the proper duty of the GSO—the general staff, in both theory and practice, was indistinguishable from ordinary staffs.<sup>68</sup>

The line of advance for GSOs through the offices reinforced the prevailing set of officer values: GSOs avoided work in the line, and their example was the envy of all others. The vast majority of officers, who possessed neither an academy nor a guards background, had little hope of advancing above company grade. In 1903, 77 percent of all captains were over forty-one years old, 32 percent were over forty-six: the gray-haired captain who had long since abandoned hope of promotion is a stock figure in the military literature. Because there was no hope of advancement, there was no incentive for zealous service or self-improvement, unless it was to prepare for entrance to the General Staff Academy and thereby to quit the line.<sup>69</sup> Advancement through the offices, then, worked against the combat efficiency of the tsarist army in two ways: the staffs attracted the most ambitious officers, effectively isolated them from the line, and identified mastery of paperwork as the key to success; line officers, with little prospect for advancement, had no reason to exert themselves.

I HAVE ARGUED THAT the duty behavior of tsarist officers was perversely unmilitary and their off-duty (in the case of theft, “extra-duty”) behavior anomic. Since these are conventional stereotypes met in the antimilitary literature of the day (not just in Russia, and for that matter not just at the turn of the century), one may justifiably be skeptical of such a characterization. Nevertheless, the writings of tsarist officers themselves confirm that these were the traits of the tsarist officer corps in the last three decades of its existence. It is easier to accept that these traits were widespread when one realizes that they did not exist in a vacuum but were the product of, or at least the complement to, the economic structure of the tsarist army. The central place of economic management in the tsarist officer’s duties discounted training in military skills and imbued officers with a bureaucratic, proprietary, and pacific mentality. The tedium and pointlessness of the established routine provided the setting for social demoralization. Other characteristics of the tsarist officer corps (the notable incivility of senior officers to their juniors, for example) also make sense within this framework, even if they were only marginally associated with the regimental economy and bureaucratic habits. The behavior of senior officers—factionalism, cliquishness,

<sup>68</sup> For particularly enlightening remarks on the theory and practice of general staff service in the tsarist army, see Bilderling, “Prokhozdenie sluzhby,” 186–93; and E. Kh. Kalnin, “General’nyi shtab i ego spetsial’nost’,” *Voennyi sbornik*, no. 4 (1907): 138–45. Also see Mayzel, “Formation of the Russian General Staff,” 312–14. For a statistical demonstration that GSOs of command rank had served overwhelmingly in administrative posts, see Oberuchev, *Nashi voennye vozhdai*, 7, 14, 23–24, 32, 53–54, 55–60. For some other comments on GSO career patterns and duties, see B. V. Gerua, *Vospominaniia*, 150–53, 196; Butovskii, *Stat’i na sovremennye temy*, 3–15; A. Gerua, *Posle voiny*, 56, 58–59; Martynov, *Iz pechal’nogo opyta*, 114–19; and Zaionchkovskii, *Samoderzhavie i russkaia armia*, 178, 181–83.

<sup>69</sup> Galkin, *Novyi put’*, 31–33; Volgin, *Ob armii*, 134; Grulev, *Zloby dnia*, 209; Pil’skii, “Armia i obshchestvo,” 43; Rezhepo, *Neskol’ko myslei*, 29; Birkin, *Osinoe gnezdo*, 217; Zaionchkovskii, *Samoderzhavie i russkaia armia*, 189.

notorious inability to coordinate operations—may be interpreted as a logical outgrowth of their experiences as junior officers and commanders of regiments (when, as proprietors of estates, they had no need for and gained no experience in coordination). Of course, not every feature of officer behavior can be linked to the regimental economy nor was the regimental economy alone responsible for officer inefficiency. The emphasis on parade-ground drill had an illustrious genealogy of its own, the role of the imperial court in filling command positions was not conducive to selection by merit, and the nature of the tsarist polity figured in the care with which generals were sheltered from criticism, embarrassment, and early retirement. Nevertheless, the regimental economy almost certainly contributed more to inefficiency in the early twentieth century than did any other feature of the tsarist military system, because it influenced the behavior of every officer.

One potentially serious objection to this analysis is that it describes a system with a built-in tendency toward equilibrium and that it therefore cannot account for or be reconciled with the historical evolution of the tsarist army. Certainly there were great improvements in the tsarist army, even in the second half of the nineteenth century—better administration, vastly improved weaponry, better officer education, even better performance of duties by officers.<sup>70</sup> What had not changed was the degree of functional specialization within the army. If in the eighteenth century the tsarist army's system of supply had been similar to that of other Continental armies, by the early twentieth century only tsarist regiments baked their own bread, procured their own meat, vegetables, and fodder, and used the soldiers' earnings in the civilian economy to purchase food. Only in the tsarist army did infantry and cavalry make their own uniforms, use their own money to purchase blankets and bed linen, repair and furnish their own barracks, and build their own summer encampments. Consequently, in no other European army did line officers have even an approximation of the management and procurement duties that tsarist officers shouldered, and in no other army were colonels and the higher staffs so intimately involved in the details of supply.<sup>71</sup> Retarded functional specialization is evident, too, in the failure of Russian GSOs to perform duties customarily associated with a general staff. Certainly the tsarist army of 1914, or even 1904, was in all material respects superior to the army of 1855. These elements of modernity, however, were set in a premodern framework. The undoubted improvements that had been introduced did not alter the pattern of a tsarist officer's duties or the criteria by which his efficiency was rated. The inefficiency of the tsarist army, and of its officer corps, inhered in the system rather than in its details.

Historians of the Russian army generally acknowledge that the years 1881–1905 were a period of stagnation. Conversely, the historiographical consensus

<sup>70</sup> See Curtiss, *Russian Army under Nicholas I*, 176–211.

<sup>71</sup> Maksheev, *Voennoe khoziaistvo*, xx–xxii, 58, 693–96. Maksheev also provided detailed descriptions of the regulations pertaining to the military economy of the French, German, and Habsburg armies. The French army had the largest number of economic similarities to the tsarist army, but the two were, on balance, vastly different; *ibid.*

holds that the tsarist army took a fundamental turn for the better in the aftermath of the Russo-Japanese War. The nine years between that war and the opening of World War I were too few for even the most far-reaching reforms to have remedied all defects, so the disaster of 1914-15 does not in itself prove that the tsarist army had made no progress. (Neither do the results of the first year of the war demonstrate, of course, that progress had been made.) An impressive array of military reforms did follow the Russo-Japanese War and impinged upon the life of the officer corps. In addition to large capital grants for the upgrading and augmenting of equipment, one sector of the service after another was reorganized or otherwise repaired, from medical deferments (broadened, so that conscripts were on average healthier) to supply trains, from the reserve system to mobilization and strategic planning. Closer to the present theme, observers reported a quickening of interest in tactics and a more serious commitment to training among many officers in the aftermath of the Russo-Japanese War. The Ministry of War seconded this initiative from below by issuing new training and combat regulations that emphasized the development of individual initiative, drill in skirmish lines, and the use of cover by infantry and artillery, all at the expense of parade-ground exercises. In 1908 the Ministry of War also ordered that junior officers be given specific training responsibilities and that they be engaged in regular discussions of tactics. Some historians have argued that by 1914 improved officer training had raised the level of professionalism of the officer corps, and others have asserted that by 1914 Russian soldiers were better trained, particularly in musketry, than those of any other army. The tsarist army's poor performance in 1914 and 1915 has been blamed, variously, on inadequate armament (because the armaments program was not given sufficient time), faulty strategic planning, and the incompetence of command personnel.<sup>72</sup>

<sup>72</sup> The traditional view, first formulated by General Nicholas N. Golovin, is that the Russian army did not have the overwhelming advantage in manpower that it was at the time thought to possess, that the Russian army was badly outgunned, and that the high command was incompetent; Minister of War Sukhomlinov (1909-15) is blamed for aborting the progressive efforts of officers such as Golovin himself. The Russian infantry and artillery, in contrast, were considered superbly trained; Golovin, *The Russian Army in the World War* (New Haven, 1931), esp. 30-44, and *The Russian Campaign of 1914: The Beginning of the War and the Operations in East Prussia* (Fort Leavenworth, Tex., 1933), 1-40. The theme of material deficiencies is also found in Soviet scholarship: K. F. Shatsillo, *Rossia pered pervoi mirovoi voinoi* (Moscow, 1974). More recently, Norman Stone has argued that in 1914 the tsarist army was not substantially inferior in material respects to the German army. Stone has emphasized the role played in Russian defeats by the generals' factionalism and their inability to coordinate strategy and operations, but he has also attempted to rehabilitate Sukhomlinov. Stone has placed undue weight both on the additional monies that were appropriated for the army after the Russo-Japanese War and on the thrust of the largely unrealized reforms that Sukhomlinov sponsored in the realm of strategic planning and troop organization. There is no evidence whatsoever to support Stone's hint that Sukhomlinov's clients were better operational planners and troop commanders than those who opposed him. In any case, it is misleading to ascribe the failure of reform to cliques and factions: the cliques were behaving in their traditional way; Stone, *The Eastern Front, 1914-1917* (New York, 1975), 18-36, *passim*. An even stronger endorsement of Sukhomlinov's activity is provided by Wilfong, "Rebuilding the Russian Army," 120-75. Allan K. Wildman, who has provided the most recent assessment of the condition of the tsarist army on the eve of World War I, has questioned Stone's views on the comparative firepower of Russia and Germany. Wildman's paradigm of tsarist military modernization—reflected most importantly in the changing social composition of the officer corps and in improvements in the officer training program but constrained by military traditionalism in the upper ranks and the traditional influence of the imperial court—is suggestive, but he has not applied this paradigm to the question of supply and functional specialization. Part of Wildman's analysis relies on Kenez's research; Wildman, *The End of the Russian Imperial Army: The Old Army and the Soldiers' Revolt*,

In gauging the impact of this flurry of reform on officer behavior, one has to keep in mind that all of the failings discussed earlier—drunkenness, theft, disengagement from training—are amply attested by sources referring to the years after 1905. Force of habit and institutional lethargy would have ensured this whether or not the post-1905 reforms had produced an immediate, palpable impact on the officer corps. Still, the mixed evidence on actual, as against officially prescribed, officer behavior indicates that attentiveness to training and tactical exercises was of brief duration and was most characteristic of units that had fought in Manchuria (one-third of all regiments); by 1914 the improvement in officer efficiency was minimal at best.<sup>73</sup> There are, for instance, more first-hand statements that regimental tactical discussions were ignored—or provided new occasions for drink—than that they served their intended purpose. Senior officers certainly evinced no interest in them.<sup>74</sup> Junior officers by and large were no more interested in drilling soldiers than before, while their seniors maintained parade-ground traditions with fierce determination and in defiance of the new regulations. Traditionalists argued that close-order drill was essential to developing that lack of concern with self that was crucial if orders were to be obeyed unthinkingly in battle. There was some truth in that, but the cast of mind that underlay such arguments was revealed when the same officers also criticized a new “Instruction for Infantry Combat” because its emphasis on the use of cover would, they claimed, teach soldiers cowardice. Orders to teach reading and writing to illiterate conscripts were disregarded because of a “lack of time.”<sup>75</sup> The artillery had a deserved reputation for exemplary gunnery, but artillery units repeated identical exercises from one year to the next, and in the opening engagements of 1914 some battery commanders out of habit drew up their guns in exposed positions. Indeed, one cavalry division commander *ordered* his artillery to draw up in the open, while the cavalry assembled in the open field as

*March–April 1917* (Princeton, 1980), 13–40, 64–74; and Kenez, “Profile of the Prerevolutionary Officer Corps,” 121–58. For other information on reforms between 1905 and 1914, see Volgin, *Ob armii*, ii, 104, 108–10, 116–17, 125, and “Dopolnenie,” iv; and V. Dragomirov, “Podgotovka russkoi armii k velikoi voine,” *Voennyi sbornik* (Belgrade), 4 (1924): 98–119, 5 (1924): 189–212, and 6 (1925): 55–76. On officers’ increased attentiveness to drill and tactics in the immediate aftermath of the Russo-Japanese War, see A. Gerua, *Posle voiny*, 334, 347; Zambrzhitskii, “Ocherki bylogo,” 19–20, and “Sapernyi bunt” [11–14]; Baranovskii, “Vospominaniia,” 54; Bitenbinder, “Vospominaniia,” chap. 5, p. 1; Stepun, *Byvshee*, 85; Rezhepo, *Neskol’ko myslei*, 8; and Ivanko, “Sluzhba v polku,” 277.

<sup>73</sup> Zambrzhitskii distinguished between the Sixth Sapper battalion in Kiev, which had seen action in Manchuria and in which the commander and most of the other officers took training seriously, and the majority of the other engineering units in Kiev, which had not gone to war and in which prewar traditions continued; Zambrzhitskii, “Sapernyi bunt” [4–6]. Mannerheim noted pointedly that a unit in which he served and in which life went on as of old had not fought in Manchuria; Mannerheim, *Erinnerungen*, 110. Almost all units for which there is explicit affirmation of improved training had been to war.

<sup>74</sup> Positive references to tactical discussions include Zambrzhitskii, “Ocherki bylogo,” 19–20; and Baranovskii, “Vospominaniia,” 54. Negative references include Bitenbinder, “Vospominaniia,” chap. 5, p. 5; Grulev, *Zloby dnia*, 37–38; Littauer, *Russian Hussar*, 17, 25, 82, 107; and Smirnov, “Ofiterskii vopros,” 146.

<sup>75</sup> Dragomirov, “Podgotovka russkoi armii,” 5: 190–93; Rezhepo, *Neskol’ko myslei*, 6–8; Mannerheim, *Erinnerungen*, 110; D. Khodnev, “Iz dorogogo proshlogo,” *Russkii voennyi vestnik* (Belgrade), January 7, 1927; Grulev, *Zloby dnia*, 54–58, 76, 78; Littauer, *Russian Hussar*, 62; Yakhontoff, *Over the Divide*, 72–75; and “Russian Regulations for Leading Infantry in Combat,” *Infantry Journal*, 8 (1911–12): 894–904. The argument over the “Instruction” was summarized by Major General Turbin, *Vospitanie v voiskakh: Disziplin, kak osnova etogo vospitaniia* [St. Petersburg, 1912], 20–28. Turbin himself was an archtraditionalist.



though for a peacetime review; as a result, the division was routed by one infantry battalion supported by four guns. The behavior of three squadron commanders in August 1914 demonstrates why the Russian cavalry had a reputation for poor reconnaissance: ordered to reconnoiter three different routes, they instead linked up to cover a single road and so reported to their corps commander, as though they had done nothing wrong.<sup>76</sup> Examples of tactical illiteracy could be multiplied endlessly; they are difficult to reconcile with the notion that the tsarist officer corps had made more than a minimal advance in efficiency between 1905 and 1914.

There may have been incremental improvement in the level of officer efficiency from 1904 to 1914, but the same can be said of the 1881–1904 period. Moreover, by 1914 efficiency may well have been declining toward rather than inching away from the level of 1904. There was precedent for this. Following both the Crimean and Russo-Turkish wars, training had briefly improved, but in both instances the old routine quickly reasserted itself.<sup>77</sup> Furthermore, just as in the past, after 1905 the natural desire to repair the defects revealed in combat took the form of a fevered hunt for a simple formula. Local fashions in training passed in rapid succession, each emphasizing one skill to the exclusion of all others. (“Every commander has his own caprice” was the way tsarist officers expressed this tendency to overdo a good thing.) Lack of system and perseverance was scarcely apt to instill in line officers the habit of consistent, professional performance of duties.<sup>78</sup>

There were two reasons, both familiar, for the survival of inappropriate habits. One was the character of general officers. A purge of the high command that followed the Russo-Japanese War (known in military circles as the “massacre of the innocents”—a nice reflection on the calibre of generals to that point) had no appreciable impact on command quality. In 1906 the government established a Higher Certification Commission to review qualifications and recommend promotion or retirement for all officers at the grade of colonel and above. In the same year the Ministry of War required special training and refresher courses for those who commanded regiments or larger formations. No doubt partly as a result of these measures, in 1906–07 between 50 and 80 percent of all troop commanders were changed (the higher the rank, the greater the turnover).<sup>79</sup> Many of those purged were indeed incompetent, but the purge seems to have been directed as much against those who had been indecisive in dealing with civilian

<sup>76</sup> On the artillery, see Galkin, “Rožn’ ili bratstvo?” 29; Kvetsinskii, “Tekhnika organizatsii manevra,” 1: 31; John Morse, *An Englishman in the Russian Ranks: Ten Months Fighting in Poland* (London, 1915), 38–39; and B. Shaposhnikov, “Nachalo mirovoi voiny,” *Voenno-istoricheskii zhurnal*, no. 6 (1967): 76–77. For a conventional assessment of the artillery’s excellence, see Dragomirov, “Podgotovka russkoi armii,” 6: 55–62. The two incidents detailed were reported by Zalesskii, *Vozmezdzie*, 66; and General Basil Gourko, *War and Revolution in Russia, 1914–1917* (New York, 1919), 22–23.

<sup>77</sup> Zaionchkovskii, *Voennye reformy*, 180–210, 356–58, and *Samoderzhavie i russkaia armia*, 253–54, 279; Butovskii, *Prezhnaia sluzhba*; and T-skii, “Zametki o konnitse,” *Voennyi sbornik*, no. 8 (1907): 63.

<sup>78</sup> Butovskii, *Novoe napravlenie*, 3–5; A. Dmitrievskii, “Ob ofiterskoi rabote,” *Voennyi sbornik*, no. 3 (1914): 89–96; A. A. Neznamov, *Tekushchie voennye voprosy* (St. Petersburg, 1909), 121; Dragomirov, “Podgotovka russkoi armii,” 4: 106–07; and D. Khodnev, “Uvlecheniia,” *Finliandets*, December 12/25, 1937, pp. 46–50.

<sup>79</sup> Denikin, *Put’ russkogo ofitsera*, 246–47; Rezhepo, *Neskol’ko myslei*, 8, 13; and Grulev, *Zloby dnia*, 275.

revolution and mutiny in 1905 and 1906 as against those with questionable ability to lead troops in battle. The Higher Certification Commission, at any rate, was pressured to find positions for incompetent but well-connected generals, while rapid advancement went to those who had acted decisively against revolution.<sup>80</sup> The incapacity of generals and their staffs in the first few months of war—their panicky flight, their unforced retreats, their failure to follow orders or seize tactical opportunities, their utter ignorance of how to conduct a war—hardly indicates that talent had been moving upward in the years before World War I.<sup>81</sup>

The promotion of such officers to command rank indicates that there was little talent in the middle grades to be culled, that the principles for judging command quality had not changed since 1904, or both. In fact, ineptitude so widespread was systemic rather than personal. Little could be expected of Russia's generals in war because little was demanded of them in peace. During the last prewar maneuvers in the summer of 1913, they repeated all of the sins of the past: fatuousness of assigned objectives (attack this, defend that), haste to complete the exercise, outright hostility toward coordinating operations with neighboring units, pre-positioning bivouacs, calculating outcomes on the basis of quantity of troops engaged rather than on position and effectiveness of fire, failure to provide soldiers with a continuous supply of shells (ignoring the fact that shells are expended in battle), failure to utilize engineering, signaling, and medical services, no appreciation of the advantages of night movement. It was perhaps just as well that junior artillery officers were, as always, excused from participation in maneuvers, since there was nothing worthwhile to learn. At the April 1914 war games for senior commanders in the Kiev military district directed by Minister of War Vladimir Sukhomlinov, the errors were repeated once again—attacks planned under wildly inappropriate conditions, no attention whatsoever paid to problems of communication and supply—and none was criticized.<sup>82</sup> Since the old routine was still acceptable at command level, there was no pressure to change routine in the regiments.

Regimental routine would in any case have been difficult to change, because the regimental economy remained largely unreformed. Beginning in 1906 some supplies that the regiments had previously purchased with their own money (blankets, bed linen, and field shirts, for instance) were issued centrally, and in the same year the Ministry of War banned the practice of hiring out soldiers for autumn work in the civilian economy. Despite these measures no more soldiers were available for drill after 1906 than before, partly because regiments contin-

<sup>80</sup> "Chistka komsostava tsarskoi armii v 1906 g.," *Krasnyi arkhiv*, 1-2 (1932): 211-25; "Iz zapisok A. F. Redigera," *ibid.*, 5 (1933): 99; Wilfong, "Rebuilding the Russian Army," 45, 66, 72; Denikin, *Staraia armia*, 1: 91-97; Wildman, *The End of the Russian Imperial Army*, 70-71; and Golovine, *Russian Campaign of 1914*, 153.

<sup>81</sup> For a catalogue of stupefying errors and egregious failures in staff work and troop dispositions, see Zaleskii, *Vozmezdie*, 145-64; and Golovine, *Russian Campaign of 1914*, 116-18, 195, 252, 237-38. Every study of Russian operations in 1914 and 1915 provides many examples of ineptitude and panic.

<sup>82</sup> Kvetsinskii, "Tekhnika organizatsii manevra," 1: 27-32, 2: 11-16; and Golovine, *Russian Campaign of 1914*, 35-39. For similar reports, see Dragomirov, "Podgotovka russkoi armii," 4: 102-03; and Sukhomlinov, *Vospominaniia*, 236-38.

ued to produce much of what they consumed. And civilian employment of soldiers was a habit that died hard: in the years just before World War I, the commander of the Sumsk Hussar regiment (Moscow) still hired out his men to pick cabbage; at the cost of cancelled drill, the regiment obtained cabbage at a discount.<sup>83</sup> Line officers may have received improved professional training by 1914, but professionalism was undermined by the established service routine, as it had always been.

A more fundamental reform—indeed, the abolition—of the regimental economy began to take shape in 1906. A military commission envisioned that the commissariat would provide for every need (either in money or in finished goods) and that money saved on purchases would revert to the government rather than augment the regimental capital (which indeed would be eliminated because regiments would no longer need working capital). There was, as noted earlier, considerable resistance to this reform, because line officers had no wish to lose the services of the regimental workshops. The greatest obstacle to reform, however, was the enormous cost that the government would incur by actually covering all of the army's needs. Nevertheless, after experiments in select units had demonstrated conclusively (and not unexpectedly) that officers and men relieved of economic chores had much more time for training, the new economic policy was officially inaugurated in 1912. But, either because money was lacking for complete implementation or for other reasons, in 1914 regiments still had their precious capital reserves and were still using them to produce a part of their own material requirements.<sup>84</sup>

A WORM'S-EYE VIEW OF OFFICER BEHAVIOR in the regiments on the eve of World War I is provided, appropriately enough, by Krasnov's memoir on the Tenth Don Cossack regiment on the Austrian border. Despite his assertion that old, militarily inefficient traditions were gone for good after the Russo-Japanese War, Krasnov found upon taking command in late 1913 that two of the six squadron commanders had for some months been withholding the soldiers' pay and remittances from home in order to cover gambling losses, that the officers were accustomed to "the quiet life" and took no great interest in training, and that his predecessor in command, a GSO, had only been marking time in the line before returning to a higher staff post, had preferred family life, cards, and the officers' mess to duty, and had not wished to vex his subordinates with unnecessary demands. There was considerable opposition among the officers to Krasnov's campaign to change the tenor of regimental life (opposition espe-

<sup>83</sup> John Bushnell, "Mutineers and Revolutionaries: Military Revolution in Russia, 1905-1907" (Ph.D. dissertation, Indiana University, 1977), 115-16; Rezhepo, *Neskol'ko myslei*, 7; Grulev, *Zloby dnia*, 154; Littauer, *Russian Hussar*, 56; Sul'menev, "Nedostatki nashego voiskovogo khoziaistva," 200; and Dragomirov, "Podgotovka russkoi armii," 5: 189, 199. In May 1917 the government considered using soldiers from the Petrograd garrison to help gather the harvest; Lobanov-Rostovsky, *Grinding Mill*, 229.

<sup>84</sup> A. Gerua, *Posle voiny*, 62; Volgin, *Ob armii*, 115-16; Grulev, *Zloby dnia*, 223, 228; Kononenko, "Novoe polozenie o khoziaistve v polku," 77-85; Iakubovskii, "Po voprosu ob uproshchenii i sokrashchenii voiskovoi denzhnoi otchetnosti," 121; and Krasnov, *Nakanune voiny*, 44.

cially, but not only, to his ban on vodka in the mess). After the officers had finally mended their ways and the Tenth Don had met Krasnov's high standards of discipline and training, their efforts went unmarked by the high command: at division review, mistakes by other regiments were ignored and praise was accorded in equal measure to efficient and inefficient regiments alike.<sup>85</sup>

Even after making allowances for the atypical character of Cossack regiments and for Krasnov's habit of presenting himself as an uncommonly charismatic commander who had everywhere to set things right, it remains the case that every one of the features of officer life that Krasnov found in the Tenth Don regiment in 1913 was reported in other units in the same period, just as they had been typical of officer life in the guards, cavalry, and line infantry in the 1880s and earlier. Russia's humiliating defeat by Japan had produced no basic shift in tsarist officer mentality. The capital grants and improved equipment that the army received on the eve of the Great War put more destructive power at the disposal of tsarist officers than they had had in the 1880s, but officers were no more able to put the men and equipment they commanded to good use than they had ever been.

<sup>85</sup> Krasnov, *Nakanune voiny*, 14, 18–19, 38–39, 53. Krasnov claimed that slanderous accounts of the tsarist army published in the Soviet Union and the West inspired his writing. "I deemed it my duty, not to take issue with it, not to dispute the slander, but to depict with photographic precision what I saw, what I experienced, and with whom I served" after assuming command of the Tenth Don regiment in late 1913; *ibid.*, 5.

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## The Limits of Suffragist Behavior: Legalism and Militancy in France, 1876–1922

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and  
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"THE RIGHTS OF MAN," wrote Parrhisia (Blanche Cremnitz) in 1906, "have triumphed by revolution. The superiority of feminism will be to secure the rights of women by evolution, without appeal to brute force."<sup>1</sup> Virtually all French suffragists shared this opinion and confined their behavior accordingly. From Hubertine Auclert's founding of the first women's suffrage society in France in 1876 to the Senate's defeat of the suffrage bill in 1922, French women adhered to a strategy of legalism: pursuit of the vote through obedience to the law and respect for bourgeois standards of behavior.

On the eve of World War I there were perhaps fifteen thousand active suffragists in France, who claimed an "avant-garde of five hundred thousand" on the basis of a 1914 newspaper poll. The French affiliate of the International Woman Suffrage Alliance, the Union Française pour le Suffrage des Femmes (UFSF), accounted for most of the movement with twelve thousand members. Parrhisia, a member of the UFSF executive committee, typified their moderation. Approximately two dozen smaller organizations also campaigned for the vote. The limited and sporadic militancy in French suffragism came from the ranks of Auclert's Suffrage des Femmes and Solidarité des Femmes. No more than two hundred and fifty women from these organizations participated in all militant activities combined. Militant demonstrations—defined as any direct action such as a parade or an outdoor rally—rarely attracted fifty participants.<sup>2</sup> Auclert went beyond strict legalism as early as 1880 (when she refused to pay

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<sup>1</sup> Parrhisia, "Pensées inédites," in *Le Petit Almanach féministe illustré* (Paris, 1906), 11.

<sup>2</sup> These figures are estimates. The largest organization working for the vote was the Conseil National des Femmes Françaises with a membership of approximately one hundred thousand. This council, however, was an association of more than one hundred women's organizations, including trade unions and philanthropic societies; most endorsed suffragism but were not active suffragists in the sense of sponsoring meetings or publishing literature. The claim of five hundred thousand suffragists was based on the number of responses to a "poll" organized by *Le Journal* in April 1914; but feminist organizations in Paris worked very hard to obtain those responses from women who had never participated in the suffrage campaign. The twelve thousand members of the UFSF (included in the one hundred thousand of the Conseil) included most members of the





*Figure 1: Hubertine Auclert (1848–1914), the founder of organized suffragism in France and a leading advocate of militant tactics. Photograph reproduced courtesy of the Bibliothèque Marguerite Durand, Paris.*

smaller organizations. Scattered records of attendance at the meetings of militant societies suggest that fifty to one hundred participants was an unusual turnout; fewer than twenty-five was the rule. Membership estimates are based on dossiers at the Bibliothèque Marguerite Durand [hereafter, BMD], in the Bouglé Collection at the Bibliothèque Historique de la Ville de Paris [hereafter, BHVP], and in the archives of the Préfecture of Police [hereafter, APP]. The BMD is an especially valuable feminist library located in the *mairie* of the Fifth Arrondissement of Paris. The Bouglé Collection is large but unclassified, making its use difficult and precise references impossible; citations are based on its status in 1979.

her taxes), but noteworthy public militancy was only attempted in France in the years 1904–08: Auclert led a small group of women in an attempt to burn a copy of the Napoleonic Code at the Place Vendôme in 1904; Caroline Kauffmann and Madeleine Pelletier of Solidarité showered the hemicycle of the Chamber of Deputies with suffragist handbills in 1906; and small parades were held during the elections of 1906 and 1908. Only when moderates cooperated in the carefully regulated *manifestation Condorcet* of July 1914 did French suffragists stage an outdoor assembly (five to six thousand) comparable to the activities of British or American suffragists. Of the few French militants, just two (Auclert and Pelletier) dared to commit acts of violence, and they did so only once.<sup>3</sup> Moderates and militants alike eschewed “brute force.” They were determined, as the Conseil National des Femmes Françaises (CNFF) stated, “to employ only dignified means and never to do anything that would render the cause ridiculous.”<sup>4</sup>

Given the French revolutionary tradition, which was esteemed as the source of *liberté* and *égalité*, given the role of women in revolutionary violence, bread rioters of 1789 or *petroleuses* of 1871, and given the lesson of how French men seized “universal” suffrage in 1848, why did French suffragists define their strategy within such cautious limits? Given the contemporary examples of British and American suffragists, such as the march of fifteen thousand women to Hyde Park in 1908, which culminated in a rally attended by possibly five hundred thousand people, why did French suffragists even consider street demonstrations improper? What restrained French behavior? The writings of French suffragists, their personal papers, and contemporary accounts suggest that the most important answer to such questions was the social origins and collective attitudes of French suffragists. Women of the liberal middle classes, with shared backgrounds and values, dominated the movement. Bourgeois *mentalités* predisposed them to gradual, lawful solutions. They recruited women of similar beliefs, and they imposed compatible attitudes on their organizations. Suffragist homogeneity tolerated latitude in ideas but little diversity in behavior. Transgressions against the shared standard of dignified means meant ostracism.

THE CONTINUING DEBATE AMONG FRENCH HISTORIANS over defining the bourgeoisie need not be resolved to realize that suffragists came from a narrow portion of the social spectrum encompassed by any definition.<sup>5</sup> Virtually no nobles

<sup>3</sup> Some details of the violent demonstrations of Auclert and Pelletier are given below. For a narrative of these events, see Steven C. Hause and Anne R. Kenney, “Women’s Suffrage and the Paris Elections of 1908,” *Laurels* (American Society of the Legion of Honor Magazine), 51 (1980): 21–32.

<sup>4</sup> Report of the suffrage section of the CNFF, *L’Action féminine*, 1 (1909): 10.

<sup>5</sup> Even Jules Michelet threw up his hands in 1844 and stated, “It is not easy to define the limits of this class, to say where it begins or where it ends”; Michelet, *The People* (Urbana, Ill., 1973), 83. For recent scholarly arguments on the question, see Alfred Cobban, “The ‘Middle Class’ in France, 1815–1848,” *French Historical Studies*, 5 (1967): 41–52; Leonore O’Boyle, “The ‘Middle Class’ Reconsidered: A Reply to Professor Cobban,” *ibid.*, 53–56; Peter Stearns, “The Middle Class: Towards a Precise Definition,” *Comparative Studies in Society and History*, 21 (1979): 377–96; Leonore O’Boyle, “The Classless Society: Comment on Stearns,” *ibid.*, 397–413; Peter Stearns, “Reply,” *ibid.*, 414–15; and Theodore Zeldin, *France, 1848–1945*, 1 (Oxford, 1973): chap. 1:

participated. The sole exception was the Duchesse d'Uzès, whose multifarious activities included membership in a moderate feminist group, Avant-Courrière, and a role in the founding of the UFSF. A light sprinkling of the aristocratic particule may be found in executive committees and editorial boards, such as Avril de Sainte-Croix (secretary-general of the CNFF) and Mme. de Montaut (a president of the Société pour l'Amélioration du Sort des Femmes).<sup>6</sup> Aristocratic women who participated in women's organizations preferred conservative Catholic groups such as the Ligue Patriotique des Françaises (affiliated with Albert de Mun's Action Libérale Populaire), which did not support women's suffrage until 1920. Conversely, Catholic organizations that considered suffragism early, such as Marie Maugeret's Féminisme Chrétien, were small bourgeois organizations attracting few aristocrats; they were also isolated from most suffragists by French national politics.<sup>7</sup>

No peasant women and few working-class women can be found among the leaders of French feminism. There is little evidence that women of the small villages and the countryside joined any political groups, although they must have accounted for a share of the immense membership of the Ligue Patriotique (over five hundred thousand in 1914).<sup>8</sup> Proletarian women seem to have accounted for only a tiny percentage of the participants in the women's movement at any level, preferring syndicalist groups or socialist organizations such as the Groupe des Femmes Socialistes.<sup>9</sup> In most cases, feminists from working-class backgrounds experienced embourgeoisement. Parrhisia, for example, was the daughter of a silk worker who had attained a managerial position and a life of middle-class comfort and values.<sup>10</sup> The marginality of the middle-class status of

"The Pretensions of the Bourgeoisie." For understanding the actions of French suffragists, the most helpful perspective is Stearns's stress upon the "values" and the "common outlook and behavior patterns" of the bourgeoisie; "The Middle Class: Towards a Precise Definition," 385-86.

<sup>6</sup> The biographies of suffragists mentioned here and subsequently are chiefly based on the dossiers at the Bibliothèque Marguerite Durand and the Bibliothèque Historique. These files contain a mixture of personal papers and newspaper clippings, including many obituaries useful in reconstructing biographies. Jane Misme regularly published short biographies in *La Française* and *Minerva*. Some helpful sketches may be found in Le Dzeh-Djen, *La Presse féministe en France de 1869 à 1914* (Paris, 1934). For d'Uzès, also see her memoirs, *Souvenirs* (Paris, 1939); and the biography by Jean Puget, *La Duchesse d'Uzès* (2d ed., Uzès, 1972). For Avril de Sainte-Croix, see her *Le Féminisme* (Paris, 1907). There are dossiers on these two women and on Mme. de Montaut at the BMD.

<sup>7</sup> For the Catholic women's movement and French suffragism, see Steven C. Hause and Anne R. Kenney, "The Development of the Catholic Women's Suffrage Movement in France, 1896-1922," *Catholic Historical Review*, 67 (1981): 11-30.

<sup>8</sup> For suffragism in the provinces, see Steven C. Hause, "The Failure of Feminism in Provincial France, 1890-1920," *Proceedings of the Western Society for French History* (forthcoming).

<sup>9</sup> The most important possible exception to this generalization was school teachers. *Institutrices* are difficult to categorize by social class, but they constituted a very large segment of the French suffrage movement and many of them came from working-class backgrounds. Generally, however, *institutrices* chose political activity in a self-selecting process of class identity: women who were most comfortable in bourgeois groups chose suffragism, while those who identified chiefly with the proletariat chose syndicalism and socialism. In any case, *institutrices* who joined the suffrage campaign did not disturb the bourgeois character of the movement. For very helpful discussions of political radicalism among teachers, see Persis C. Hunt, "Revolutionary Syndicalism and Feminism among Teachers in France, 1900-1921" (Ph.D. dissertation, Tufts University, 1975), University Microfilm no. 76-19,305, and "Teachers and Workers: Problems of Feminist Organization in the Early Third Republic," *Third Republic/Troisième République* [hereafter, *TR/TR*], 3-4 (1977): 168-204. Also see the comments on the role of *institutrices* in Jane Misme, *Pour le suffrage des femmes: Le Féminisme et la politique* (Paris [ca. 1909]), 9.

<sup>10</sup> Manuscript biography in the Dossier Blanche Cremlitz, BMD.

such women probably reinforced their attachment to bourgeois values: the life of "brute force" was the world that Parrhisia's family had left behind.

The most important French feminists who rose from humble origins were Eliska Vincent, Marie Bonnevial, and Madeleine Pelletier. Vincent, who founded the militant society *Égalité* in 1888, was a Communard and the daughter of a Cabetist artisan who participated in the revolution of 1848. Bonnevial, a president of the *Ligue Française pour le Droit des Femmes* (LFDF), was born to a family of "very modest" means and raised by an uncle who worked as a wheelwright. Her radicalism as a young teacher during the government of moral order, especially her advocacy of laic education, cost Bonnevial her job and led to a voluntary exile. Pelletier, the most revolutionary feminist of the Third Republic, was the daughter of very poor fruit and vegetable sellers.

Even these three militants were affected by the bourgeois life of French suffragists. Vincent retained a radical commitment to equality all of her life, but she became a wealthy landowner and fit the role of a grand dame of bourgeois feminism. She became so respectable in dress, rhetoric, and deportment that she proved an acceptable speaker at Catholic women's meetings and a popular choice as the figurehead president of the UFSF. Bonnevial likewise modulated her youthful ideas and fit well into middle-class radicalism, which had made her vision of laic education the national orthodoxy. She joined a mixed masonic lodge, established by Maria Deraismes, and the *Ligue des Droits de l'Homme*—both bastions of respectable radicalism. By 1913, Bonnevial was comfortable in the CNFF (the most moderate of feminist groups) and could tell their national congress that militant tactics may once have been necessary to awaken opinion but that that time had passed. Pelletier, whom some considered bourgeois because she was one of the few women physicians in France, never chose respectability. Quite the opposite: she remained an outcast on the fringes of the suffrage movement, utterly shocking many feminists with her masculine attire, her advanced ideas such as the right to abortions, and her revolutionary socialism. Her occupation never provided her with the affluence that she observed as a characteristic of most suffragists, but her practice did produce an independent life with an apartment in the Fifth Arrondissement and a second home in the country.<sup>11</sup>

<sup>11</sup> For Vincent, see *Dossiers Eliska Vincent and UFSF*, BMD. For Bonnevial, see *Dossier Marie Bonnevial*, BMD; and Charles Sowerwine, *Les Femmes et le socialisme* (Paris, 1978), 76–78 (a revised edition of this important work is forthcoming from Cambridge University Press under the title *Sisters or Citizens?*); and, for Bonnevial's renunciation of militancy, see Avril de Sainte-Croix, ed., *Dixième congrès international des femmes: Oeuvres et institutions féminines ... le 2 juin 1913 ... compte rendu* (Paris, 1914), 320. The information on Pelletier is particularly rich. Her dossier at the BMD includes a short diary and an exceptionally poignant autobiography, which she dictated while incarcerated in an asylum in 1939. Her correspondence with Arria Ly, at the BHVP, is extraordinarily candid. For discussion of Pelletier, see Sowerwine, *Les Femmes et le socialisme*, 125–43; Anne R. Kenney, "A Militant Feminist in France: Dr. Madeleine Pelletier, Her Ideas and Actions," paper delivered at the Fourth Berkshire Conference on the History of Women, Mount Holyoke College, August 24, 1978 (copies deposited at the Schlesinger Library and the archives of Mount Holyoke College); and Marilyn Boxer, "Socialism Faces Feminism in France, 1879–1913" (Ph.D. dissertation, University of California, Riverside, 1975), University Microfilm no. 76-12,645. The chief arguments of Boxer's thesis are nicely recapitulated in her "Socialism Faces Feminism: The Failure of Synthesis in France, 1879–1914," in Boxer and Jean H. Quataert, eds., *Socialist Women: European Socialist Feminism in the Nineteenth and Early Twentieth Centuries* (New York, 1978), 98–105.



Figure 2: Dr. Madeleine Pelletier (1874–1939), militant suffragist leader of *Solidarité des Femmes*. Photograph reproduced courtesy of the Bibliothèque Marguerite Durand, Paris.

The few aristocrats or workers in French suffragism were far outnumbered by wealthy women of the upper middle class. If a dozen, or even two or three dozen, prominent nonbourgeois women could be identified, they would still be hidden behind the well-to-do. One feminist estimated in 1910 that the CNFF alone included forty to fifty millionaires.<sup>12</sup> Most of these women were born to wealth and came to feminism gradually, via philanthropy. Groups such as the *Conférence de Versailles*, which brought together women of the Protestant elite during the 1890s, became bridges between charity work and moderate feminism. Mme. Alphen-Salvador (a secretary of the CNFF), for example, belonged to a wealthy Touraine family. Her founding of a school for nurses in 1900 led her to the *Conférence de Versailles*. When leaders of that group established the CNFF in 1901, Alphen-Salvador joined; eight years later, she founded the first provincial branch at Tours.

<sup>12</sup> Héra Mirtel, "Féminisme mondial," *Documents du progrès*, May 1910, p. 436.



While wealthy middle-class women were especially concentrated in the CNFF, they were active throughout French suffragism. Cécile Brunshawicq, the secretary-general and driving force of the UFSF, was the daughter of a prosperous Alsatian industrialist, Arthur Kahn. Marguerite Durand, the founder and editor of *La Fronde*, published at a considerable loss this feminist daily newspaper and later absorbed the costs of a parliamentary campaign by backing her feminism with her personal wealth. Even the founder of militant suffragism in France, Auclert, though far removed from the millionaires of the CNFF, was the daughter of a landed *propriétaire* in Allier and lived in Paris for over forty years on her inherited *rentes*.<sup>13</sup>

Such middle-class wealth was common among French suffragists, but these women constituted only an important minority. Women from the liberal professions dominated the movement. By birth, by marriage, and by their own careers, the world of French suffragists was filled with lawyers, journalists, physicians, artists, professors, novelists, Protestant clergy, and teachers at all levels. The legal profession provides a vivid illustration.<sup>14</sup> Despite the late opening of the profession to women—Jeanne Chauvin was the first woman to receive a law degree, in 1897, and Eugénie Petit was the first admitted to the bar, in 1904—by 1914 all of the large suffrage leagues had lawyers in their leadership. The most important was unquestionably Maria Vérone, the secretary-general of the LFDF. Vérone, who took several feminist cases to court such as the Halbwachs (voter registration) Case of 1914, was also married to a prominent attorney, Georges Lhermite, who in turn was a vice-president of the LFDF. The executive council of that league included a third lawyer, Hélène Miropolski, who had graduated with Vérone in 1904. Petit was a member of the executive council of the CNFF. Three lawyers, Suzanne Grinberg, Pauline Rebour, and Marcelle Kraemer-Bach, were on the central committee of the UFSF. Chauvin was one of the organizers of the Congrès des Oeuvres et Institutions Féminines, which led to the creation of the CNFF, in 1900. Several other feminists—including Isa-

<sup>13</sup> For the CNFF and the Conférence de Versailles, see *La Femme* (the organ of the Conférence), *passim*—for example, “CNFF,” *La Femme*, 23 (1901): 42–43 (on the founding of the CNFF); and Avril de Sainte-Croix, *Le Féminisme*, 146–67. Much information is available at the Bibliothèque Marguerite Durand in the dossiers on the leaders of the CNFF (especially Avril de Sainte-Croix, Isabelle Bogelot, Sarah Monod, and Julie Siegfried), on the council itself, and on the congress that preceded its creation (Dossier Congrès International des Oeuvres et Institutions Féminines). For Alphen-Salvador, see “Figures d’autrefois et d’aujourd’hui: Mme Alphen-Salvador,” *La Française*, September 1, 1907; and “Le Conseil national à Tours,” *ibid.*, November 22, 1908. For Brunshawicq, see Jean Jolly, ed., *Dictionnaire des parlementaires français: Notices biographiques sur les ministres, députés et sénateurs français de 1889–1940*, 8 vols. (Paris, 1960–77), 2: 799–801; and Dossier Cécile Brunshawicq, BMD and BHVP (Dos 114). For Durand, there are many primary sources at the BMD; also see Sue H. Goliber, “The Life and Times of Marguerite Durand” (Ph.D. dissertation, Kent State University, 1975), University Microfilm no. 76-14,368. Goliber has speculated that Durand’s money actually came from her lover(s); *ibid.*, 47–49. For Auclert, see her newspaper, *La Citoyenne* (1881–91); her column “Le Féminisme,” in *Le Radical* (1896–1909); and her books, especially *Le Vote des femmes* (Paris, 1908) and *Les Femmes au gouvernement* (Paris, 1923). There are large dossiers on Auclert, including correspondence and a diary, at the Bibliothèque Marguerite Durand and Bibliothèque Historique and in the archives of the Préfecture of Police (B<sup>A</sup> 885).

<sup>14</sup> Similar illustrations could be drawn from most professions. For example, Pelletier and Blanche Edwards-Pilliet (LFDF) were doctors; Jeanne Schmahl (UFSF) studied medicine; Louise Bodin (*La Voix des femmes*) and Marie Georges-Martin (CNFF) were wives of physicians.

belle Bogelot, the founding president of the CNFF, and Jeanne Chenu, the leader of Action Sociale de la Femme—were the wives of attorneys.<sup>15</sup>

The importance of middle-class homogeneity in French suffragism derives not from occupation or income but from collective attitudes. Suffragists graphically illustrate André Siegfried's assertion that the French bourgeoisie could best be characterized by its attitudes, behavior, and mores.<sup>16</sup> Two pillars of bourgeois respectability dictated the limits of political behavior: maintaining dignity and avoiding ridicule. Suffragists candidly admitted their preoccupation with appearances and proper behavior, particularly when contemplating any form of direct action.

Illustrations of this attitude are legion. As one periodical put it in 1910, "Our compatriots are not, unlike the English women, impermeable to ridicule."<sup>17</sup> Jeanne Schmahl, the founder of the UFSF, cited the danger of ridicule in explaining why she could not support a woman candidate in parliamentary elections. Jeanne Oddo-Deflou, the president of the Group Française d'Études Féministes, used precisely the same argument a few years later.<sup>18</sup> Durand, arguably the most influential French feminist of her generation, also stressed the need to avoid ridicule. A suffragette campaign in France, she said, "would quickly be stopped by ridicule."<sup>19</sup> Jane Misme, who succeeded Durand at the head of feminist journalism, agreed. Suffragist behavior, according to Misme, was based on "an innate caution in the French temperament." Her newspaper, *La Française*, absolutely opposed any form of "violent public demonstrations" as "essentially incompatible with French style [*goût*]."<sup>20</sup> Another suffragist tried to explain this attitude to *The Times* of London: French women rejected the "over-zealous" behavior seen in England, in favor of "more discreet" methods that were compatible with their sense of dignity.<sup>21</sup> Preparing to found the UFSF with a series of articles in *La Française*, Schmahl defined a suffragist as one who "will instinctively avoid everything that could shock or alarm," a strategy that she labeled "essentially bourgeois." A few weeks later, Schmahl inscribed the word "dignity" prominently in the union's statutes.<sup>22</sup> This emphasis on middle-class re-

<sup>15</sup> For Vêrone, see the memorial issue of *Le Droit des femmes* (June 1938) and the dossiers on her and the LFDF at the Bibliothèque Marguerite Durand. There are also dossiers on Rebour (BMD, BHVP), Grinberg (BHVP), and Kraemer-Bach (BMD). Also see the dossiers on the appropriate groups: LFDF, UFSF, and CNFF (BMD) and the Dossier Groupements Divers (BHVP).

<sup>16</sup> Could it be unimportant in the development of Siegfried's thought that his mother was a president of the CNFF and he grew up in a home filled with feminine and feminist meetings? For his experiences with French feminism, see his *lettre-préface* to Louli Sanua (Milhaud-Sanua), *Figures féminines, 1900-1939* (Paris, 1949), 15-16.

<sup>17</sup> F. Dupin de St. André, "Les Suffragettes anglaises," *La Femme*, 32 (1910): 164.

<sup>18</sup> Hubertine Auclert, "Le Féminisme," *Le Radical*, January 3, 1902, and "L'Agitation féministe électorale," *Le Journal des femmes*, May 1910.

<sup>19</sup> "Candidate Marguerite Durand," *L'Intransigeant*, February 6, 1910. For similar remarks by Durand on ridicule and good behavior, see "Une Campagne féministe," *La Liberté*, February 10, 1910; Dossiers Vote des Femmes, 1910-13, and Féminisme-France, 1910-19, BMD.

<sup>20</sup> "Les Suffragettes anglaises," *La Française*, June 28, 1908. Similar attitudes may be found throughout *La Française*—for example, Jane Misme, "Messieurs les suffragettes," April 14, 1912, and "Les Françaises et les démonstrations de la rue," April 4, 1914.

<sup>21</sup> "The Feminist Movement in France," *The Times* (London), June 3, 1908.

<sup>22</sup> Jeanne Schmahl, "Propos d'une suffragiste," *La Française*, February 7, 1909. The statutes obliged members "to fulfill with dignity their duties." Dossier UFSF, BMD, which has been excerpted at length in André Leclère, *Le Vote des femmes en France: Les Causes de l'attitude particulière de notre pays* (Paris, 1929), 85-90.

spectability had tremendous appeal in the provincial cities where the UFSF grew rapidly. As Marguerite Clément said in establishing the branch at Bordeaux, "We do not want to be seen as ridiculous, not in dress, in manners, or in claims."<sup>23</sup> Even French militants shared these attitudes. When Auclert and Kauffmann proposed the demonstration at the Place Vendôme to the members of Suffrage des Femmes, most refused "to brave the ridicule."<sup>24</sup> And, when Madeleine Pelletier suggested in 1908 that the women of Solidarité break the windows of a polling place, they were shocked that she would actually behave that way.<sup>25</sup>

Such evidence cannot be ignored: awareness of the bourgeois composition of French suffragism with the standards of behavior that it entailed is essential to understanding the strategy of legalism. But, as an explanation, it is insufficient. Around the world, women's suffrage was a bourgeois phenomenon, led by women of education and leisure who recognized the gap between their socioeconomic status and their political position.<sup>26</sup> All of the affiliated groups in the International Woman Suffrage Alliance, such as Millicent Fawcett's National Union of Women's Suffrage Societies (NUWSS) in Britain, Carrie Chapman Catt's National American Woman Suffrage Association (NAWSA) in the United States, Anita Augspurg's Deutscher Verband für Frauenstimmrecht in Germany, or Zinaida Mirovich's Women's Union in Russia, were liberal, middle-class societies.<sup>27</sup> A survey of the American suffragists listed in *Notable American Women*, for example, shows that they came overwhelmingly from the well-educated middle class. Similarly, the questionnaires completed by participants in the All-Russian Women's Congress of 1908 show the movement there was thoroughly bourgeois and professional.<sup>28</sup> Nor were French suffrage groups the only ones led by lawyers. Augspurg received a legal education in Zurich. Catt had some legal training, as did one of the most prominent American mili-

<sup>23</sup> Clément, *Conférence sur le suffrage des Femmes* (Paris, 1912), 3.

<sup>24</sup> Police report, October 15, 1904, Dossier Vote des Femmes, APP, B<sup>A</sup> 1651.

<sup>25</sup> Pelletier, "Le Féminisme et ses militants," *Documents du progrès*, July 1909, 24. Pelletier used the word *pusillanimité* to describe French feminists and criticized their dread of "compromising the famous feminine respectability and fear of ridicule"; *ibid.*, 23-24.

<sup>26</sup> For good introductions to the comparative history of international feminism, see Richard J. Evans, *The Feminists: Women's Emancipation Movements in Europe, America, and Australasia, 1840-1920* (New York, 1977); and Ross E. Paulson, *Women's Suffrage and Prohibition: A Comparative Study of Equality and Social Control* (Glenview, Ill., 1973). For suggestions for changing the focus of such comparisons, also see Charles Sowerwine, "The Organization of French Socialist Women, 1880-1914: A European Perspective for Women's Movements," *Historical Reflections*, 3 (1976): 21-23; and, for the analysis of American suffragism as a bourgeois movement, see Aileen S. Kraditor, *The Ideas of the Woman Suffrage Movement, 1890-1920* (1965; Anchor edn., New York, 1971), chap. 9: "Woman Suffrage in Perspective."

<sup>27</sup> For more on the bourgeois character of these suffrage movements, see Constance Rover, *Women's Suffrage and Party Politics in Britain, 1866-1914* (London, 1967), 12-17; Andrew Rosen, *Rise Up Women! The Militant Campaign of the Women's Social and Political Union, 1903-1914* (London, 1974), 7-9; William L. O'Neill, *Everyone Was Brave: The Rise and Fall of Feminism in America* (Chicago, 1969), 146-68; Eleanor Flexner, *Century of Struggle: The Woman's Rights Movement in the United States* (Cambridge, Mass., 1966), 179, 216-19; David Morgan, *Suffragists and Democrats: The Politics of Woman Suffrage in America* (East Lansing, Mich., 1972), 24; Richard J. Evans, *The Feminist Movement in Germany, 1894-1933* (London, 1976), 1-3; and Richard Stites, *The Women's Liberation Movement in Russia: Feminism, Nihilism, and Bolshevism, 1860-1930* (Princeton, 1978), 213-18, 227-28.

<sup>28</sup> Anne F. Scott and Andrew M. Scott, "The Suffragists: A Collective Sketch," in their *One Half the People: The Fight for Woman Suffrage* (Philadelphia, 1975), 164-65. Also see Robert E. Riegel, *American Feminists* (Lawrence, Kans., 1963), chap. 7: "Professional Women," 113-36; and Stites, *The Women's Liberation Movement in Russia*, 216-17.

tants, Harriot Stanton Blatch. Blatch's co-founder of the Equality League (which popularized the very tactics French women rejected), Inez Milholland, held a law degree from New York University. Richard Pankhurst was a barrister, and his famous militant daughter, Christabel, read the law at Victoria University in Manchester. The Pankhursts' Women's Social and Political Union (WSPU) also demonstrated that middle-class affluence was no barrier to militant behavior. Emmeline Pankhurst was the daughter of a well-to-do Manchester manufacturer, and the WSPU drew heavily upon the wealth of the Pethick-Lawrences for several years.<sup>29</sup>

A closer examination of militant suffragism in Britain (and, to some extent, in the United States) reveals some differences in social composition and significant differences in the source of collective attitudes. The WSPU was unmistakably bourgeois during the period of its greatest notoriety (1906–14). The origins of the union, however, were closely associated with working-class politics in Manchester. Many of the early leaders and a large proportion of the founding members were workers. More important, the Pankhursts learned their militant tactics from their involvement in socialist politics. The early WSPU (1903–06) merely duplicated the behavior that members had found necessary and effective while working in the Independent Labour Party (ILP). A strategy of public protest and demonstration was, therefore, well established before the WSPU became a middle-class organization.

Emmeline and Richard Pankhurst were both ILP candidates for office in the 1890s, and Emmeline rose to the ILP's National Administrative Council by 1904. The first time that Emmeline and Christabel Pankhurst were arrested had nothing to do with women's suffrage: in 1896 they participated in a Manchester ILP demonstration over the city council's decision to close a local park to ILP meetings. At a time when international suffragism was essentially a movement of salon and drawing room, with polite lectures in rented halls, the Pankhursts were learning the advantages of parades and outdoors assemblies—a natural form of protest in a movement that could not always afford rentals. The Pankhursts brought the working-class experience to suffragism, drawing upon very different group attitudes from those the French suffragists did. Indeed, the Pankhursts initially conceived of the WSPU as a working-class organization, which they planned to name the Women's Labour Representation Committee. The majority of the founders of the WSPU were workers affiliated with the ILP. The principal speakers for the WSPU included Annie Kenney (a cotton factory worker), Hannah Webster Mitchell (a maid and a seamstress), Teresa Billington (a teacher and the daughter of an iron foundry clerk), and Flora Drummond (a Scottish worker).<sup>30</sup>

<sup>29</sup> Evans, *The Feminist Movement in Germany*, 42; Scott and Scott, *One Half the People*, 30–32; and Rosen, *Rise Up Women*, 14–15, 62, 69.

<sup>30</sup> See Rosen, *Rise Up Women*, chaps., 2–3. Also see David Morgan, *Suffragists and Liberals: The Politics of Woman Suffrage in England* (Oxford, 1975), 159: "Mrs. Pankhurst brought from her more recent working class association the more desperate, defiant flavour of lower class activism and added her own sense of drama and immediacy." But a recent history of Lancashire working women in suffragism de-emphasizes the roots of the WSPU in the labor movement due to its subsequent bourgeois character; see Jill Liddington and Jill Norris, *One Hand Tied Behind Us: The Rise of the Women's Suffrage Movement* (London, 1978).

When the WSPU began its militant campaign in 1905–06, it was still largely a working-class organization. In the march of February 1906, which launched WSPU activity in London, the largest contingent of demonstrators was the group of three hundred workers recruited by Sylvia Pankhurst in the East End of London. Few workers remained when militancy turned to violence in the window-breaking campaign in June 1909; but, as Sylvia Pankhurst always maintained, her work with laboring women in the East End resulted from the family tradition that Christabel abandoned.<sup>31</sup>

Militant suffragism in the United States did not have such direct roots in working-class politics, yet it had a closer connection than French suffragism did. American suffragists perceived direct action as a working-class tactic, as Catt testified to the Judiciary Committee of the House of Representatives. The source of this behavior in the United States, however, was a close connection between American militants and the WSPU. Blatch introduced large suffragist demonstrations in America after returning from twenty years in England; Milholland embraced English tactics because they coincided with her experience as a socialist labor organizer—New York City strikes had taught her the value of activity in the streets. Alice Paul and Lucy Burns, whose Woman's Party staged the most dramatic demonstrations in the United States, both belonged to the WSPU and served time in prison with the Pankhursts. Thus, the root of militant demonstrations in American suffragism was at least indirectly working-class politics. Militants recruited members who accepted such tactics. Blatch dedicated the Equality League (of Self-Supporting Women) to the "drawing of industrial women into the suffrage campaign"; the Woman's Party, though fundamentally middle class, had a larger proportion of working women than did French suffrage groups.<sup>32</sup>

Continental European suffragism was significantly different. German suffragists avoided street demonstrations and parades precisely because these were socialist tactics. Even the calm marches of British moderates were unacceptable to Continental bourgeois women who grasped the implications of class war in this behavior: did not Rosa Luxemburg say activity in the streets was a rehearsal for the coming revolution? Visiting London in 1908, Augspurg wrote back, "What is known here as moderate would still be the summit of outrageousness in Germany." Simultaneously, the German socialist women's movement rejected collaboration with bourgeois feminism: why should workers aid suffragists who merely wanted reforms to preserve their world?<sup>33</sup> The same thing happened in

<sup>31</sup> Rosen, *Rise Up Women*, 49–57; Morgan, *Suffragists and Liberals*, 159–60; and E. Sylvia Pankhurst, *The Suffragette Movement: An Intimate Account of Persons and Ideas* (1931; Virago edn., London, 1977), 116–70, esp. 168. And see the introduction by Sylvia's son, Richard, *ibid.*

<sup>32</sup> Paulson, *Women's Suffrage and Prohibition*, 156; Flexner, *Century of Struggle*, 249–59; and Scott and Scott, *One Half the People*, 29–31. Also see the history of the Woman's Party's militancy: Doris Stevens, *Jailed For Freedom* (1920; Shocken edn., New York, 1976).

<sup>33</sup> Augspurg, as quoted in Evans, *The Feminist Movement in Germany*, 89. In general, see Evans, *The Feminist Movement in Germany*, 87–92; and Jean H. Quataert, *Reluctant Feminists in German Social Democracy, 1885–1917* (Princeton, 1979), 3, 93, 231. For a statement of the Marxist position, see Werner Thönnessen, *The Emancipation of Women: The Rise and Fall of the Women's Movement in German Social Democracy, 1863–1933* (London, 1973), esp. 39–71.



Italy and Russia. "Between the emancipated woman of the intelligentsia and the toiling woman with calloused hands," wrote the prominent Bolshevik Alexandra Kollontai, "there was such an unbridgeable gulf that there could be no question of . . . agreement between them." Indeed, Bolshevik women were instructed not to participate in bourgeois feminist activities unless they could take control of them or disrupt them. Kollontai tried this tactic at the founding meeting of the Women's Union, provoking one feminist to respond, "Strangling is too good for you!"<sup>34</sup>

The isolation of bourgeois suffragism from proletarian women and politics was especially pronounced in France. There had been a long-established theoretical connection between utopian socialism and feminism when Auclert (who was not a socialist) proposed cooperation between suffragists and socialists in 1879. She asked the Parti Ouvrier Français at the Congress of Marseilles to establish "a pact of alliance . . . against our common oppressors." This produced a socialist motion in favor of women's suffrage, but Auclert quickly realized that the party did not wish to work for women's rights.<sup>35</sup> Over the next forty years, French socialists kept a good record of verbal support for women's suffrage, a poor record of actual work for the vote, and a hidden record of opposition to the reform. This was true of Jaurèsian moderates, who postponed women's suffrage until women and the republic were ready for it, doctrinaire Marxists, who argued that reformism detracted from work toward the revolution that would produce full equality, and Hervist antiparliamentarians, who saw no point in gaining admission to a body that they sought to abolish. The nature of suffragism led to such conclusions: it stressed the interests of sex rather than those of class, it concentrated on political rather than economic equality, and it sought to reform and preserve the Radical republic rather than to create the social republic.<sup>36</sup>

In 1880, French suffragists approached Jules Guesde to support the parliamentary candidacy of Léonie Rouzade; he declined to accept "an encumbrance" for the party. The candidacy of Paule Mink in 1893 met a similar

<sup>34</sup> Kollontai, as quoted in Stites, *The Women's Liberation Movement in Russia*, 228. Also see Stites, *The Women's Liberation Movement in Russia*, 191, 203, 210, 213, 217–18, 222. For the Bolshevik position against collaboration, see S. Serditova, *Bolsheiki v borbe za zhenskii proletarskie massy (1903–Febral 1917 g.)* (Moscow, 1959), 51–55. And, for feminist-socialist relations in Italy, see Franca P. Bortolotti, *Socialismo e questione femminile in Italia, 1892–1922* (Milan, 1974), esp. 36–57.

<sup>35</sup> Auclert, as quoted in Sowerwine, *Les Femmes et le socialisme*, 15. The relationship between French socialism and feminism during this period has been quite well examined by Charles Sowerwine and Marilyn J. Boxer. For Sowerwine's conclusions, see his *Les Femmes et le socialisme*, 233–41. Also see his "Le Group féministe socialiste, 1899–1902," *Le Mouvement social*, no. 90 (1975): 87–120, and "Women and the Origins of the French Socialist Party," *TR/TR*, 3–4 (1977): 104–27. Boxer has reached similar conclusions: a synthesis of feminism and socialism "remained an ideal and an illusion"; "Socialism Faces Feminism," 107. Also see her "French Socialism, Feminism, and the Family," *TR/TR*, 3–4 (1977): 128–67. For the efforts of Auclert, Léonie Rouzade, and others to establish a suffragist-socialist alliance in the late nineteenth century, see Sowerwine, *Les Femmes et le socialisme*, 14–42. The best work for suffragism during these years is Patrick K. Bidelman, "The Feminist Movement in France: the Formative Years, 1858–1889" (Ph.D. dissertation, Michigan State University, 1975), University Microfilm no. 76-5,521, esp. chap. 4: "Conflict: Hubertine Auclert and the Strategy of *L'Assaut*," 182–271. A revised edition of this helpful work is forthcoming from Greenwood Press, under the title *Pariahs Stand Up! The Founding of the Liberal Feminist Movement in France, 1858–1889*.

<sup>36</sup> Stites has stressed this factor in explaining the gulf between bourgeois feminism and working women in Russia; *The Women's Liberation Movement in Russia*, 228.

fate.<sup>37</sup> Had French socialists truly wished to press for women's political rights, Pelletier gave them an excellent opportunity during the 1910 elections. By that date, the unified party (SFIO) held thirteen safe seats in the Paris region, four of which were contested by new candidates. Pelletier argued that her victory in one of these constituencies, particularly if repeated should the government nullify the results, would force the issue. The SFIO did endorse Pelletier's candidacy, on the condition that she abandon her projected campaign as a feminist in the Fifth Arrondissement and run a hopeless race in the monarchist Eighth Arrondissement. Thereafter, the party gave her no help. Kauffmann, also a socialist candidate in 1910, lamented that the party would not even rent a hall for her to make a single speech. *L'Humanité* paid scant attention to these suffragist races, but made socialist attitudes abundantly clear with a single sentence on Pelletier's campaign: "In this reactionary quarter, Citizeness Madeleine Pelletier propagates socialist doctrines and *her* feminist theories." If the message were not clear, the party soon put it bluntly: Pelletier could have an administrative post in the SFIO if she abandoned the feminist movement and ceased publishing *La Suffragiste*.<sup>38</sup>

The socialist women's movement in France also scorned collaboration with bourgeois suffragism. Women of the SFIO (perhaps 3 percent of the party) might have brought revolutionary rhetoric and direct action to French suffragism, but this rarely happened. Those who aspired to be both feminists and socialists ultimately faced a choice: did they put their feminism or their socialism first? For republican socialists (outside the SFIO), such as Vêrone and Bonneval, the answer was easy. They were feminists who would vote socialist on the day they won the ballot. Most socialist women put the party first. Louise Saumoneau, who dominated the Groupe des Femmes Socialistes, insisted upon this. She bluntly rejected collaboration: "The women of the people . . . must not abandon the terrain of class struggle . . . to run after a chimerical . . . emancipation on the bourgeois terrain."<sup>39</sup> Saumoneau characterized bourgeois feminists as "intriguing, naïve, deranged, and hysterical women," with whom "there can exist no line of solidarity."<sup>40</sup>

Much of the isolation of French suffragism resulted from the bourgeois character of the movement. When Saumoneau first contemplated political activity, she considered joining Solidarité, which espoused some socialism. Sacrificing an afternoon of work as a seamstress to attend a meeting, she was appalled to find

<sup>37</sup> Boxer, "Socialism Faces Feminism," 79–80, 85–86; and Sowerwine, *Les Femmes et le socialisme*, 20–24, 70–75.

<sup>38</sup> Madeleine Pelletier, "Ma Candidature à la députation," *Documents du progrès*, July 1910, pp. 11–19; text of electoral speech by Pelletier, April 23, 1910, Dossier Madeleine Pelletier, BMD; Caroline Kauffmann to Arria Ly, May 17, 1912, BHVP; and "La Bataille socialiste," *L'Humanité*, April 19, 1910 (italics added).

<sup>39</sup> "Tribune féministe," *Le Petit sou*, October 19, 1900, as quoted by Sowerwine, "The Organization of French Socialist Women," 10. For more on Saumoneau, see *La Femme socialiste* (which she edited), especially a series of articles during 1913 and 1914 on the impossibility of cooperation with bourgeois suffragists. A similar series of articles appeared in *L'Équité* during 1913. Many of these articles are excerpted at length in Maité Albistur and Daniel Armogathe, eds., *Le Grief des femmes: Anthologie de textes féministes du second empire à nos jours*, 2 (Paris, 1978): 115–23. For a study of Saumoneau, see Sowerwine, *Les Femmes et le socialisme*, 146–67.

<sup>40</sup> Saumoneau, "Mouvement féministe, socialiste et prolétarien," *La Femme socialiste*, September 1, 1913, and "Le Mouvement féministe-socialiste," *ibid.*, October 1901.

the group earnestly discussing dowries. Saumoneau concluded that feminists came from the class of employers, not of workers; she never returned. Later attempts to bring French working-class women and feminists together, notably an effort by Durand, failed completely. The chasm separating the two groups was dramatically revealed during the debates at feminist congresses at Paris in 1900 and 1908. Despite heated socialist appeals, feminists refused to give their maids a day off.<sup>41</sup>

The bourgeois character of international suffragism, therefore, clearly had national variations. In Continental states, where the class struggle was more sharply perceived, suffragists faced a different situation from the one that their British and American colleagues did. Middle-class insistence upon respectable behavior was indubitably a powerful factor in conditioning suffragist moderation everywhere. Yet it was doubly important for French women: it deprived them of a militant leavening and experience that British suffragists shared, and it prevented public demonstrations because they were too closely associated with revolutionaries.

NUMEROUS OTHER FACTORS CONTRIBUTED to French moderation. It is important to note that militancy developed in Britain and the United States through splinter organizations that appeared long after middle-class moderates had established a large suffrage campaign. In the United States the NAWSA had seventy-five thousand members and a half century of suffragist experience before the Equality League and the Woman's party appeared. Fawcett's National Union was a huge coalition of four hundred societies with a long history of activity when the WSPU seized the world's attention. In contrast, the UFSF was an infant before World War I, its membership only a small fraction of that of the NAWSA or the NUWSS. The French union was still in its age of growth and consolidation when militant fragmentation occurred in the United States and Britain. Hence, some explanation of French behavior may be found by examining the pattern of feminist development.

In countries where suffragism developed rapidly, the movement profited from experiences unparalleled in France. British and American women received political education, for example, through the Anti-Corn Law League or the abolitionist movement.<sup>42</sup> No comparable campaign involved French women. British and American women converted their experience into large women's organizations, which further taught them the art of political pressure. Josephine Butler's association for the repeal of the Contagious Diseases Acts in Britain or the Women's Christian Temperance Union (WCTU) in other countries provided great advantages to nascent suffrage groups.<sup>43</sup> Suffragists inherited a large, well-

<sup>41</sup> Sowerwine, *Les Femmes et le socialisme*, 65-87.

<sup>42</sup> See Millicent Fawcett, *Women's Suffrage: A Short History of a Great Movement* (reprint edn., London, 1970), 13; Rover, *Women's Suffrage and Party Politics in Britain*, 61-62; Rosen, *Rise Up Women*, 8-9; and Flexner, *Century of Struggle*, 78-104.

<sup>43</sup> The tremendous advantage to suffragism of a large WCTU is perhaps best seen in the case of New Zealand. See Patricia Grimshaw, *Women's Suffrage in New Zealand* (Auckland, 1972), esp. 21-35. For the similar role of the WCTU in other English-speaking countries, see Paulson, *Women's Suffrage and Prohibition, passim*; Catherine Cleverdon, *The Woman Suffrage Movement in Canada* (Toronto, 1950), 11; D. Scott, "Woman Suffrage: The

organized base for their efforts and drew converts who believed that women would vote to reform prostitution in Britain, drinking habits in New Zealand, or immigration laws in the United States. These suffrage movements received a further impetus when national reforms of the male franchise excluded women: the extension of the vote to exslaves in America, the Reform Bill of 1867 in Britain. (In other places—Finland, Norway, Bohemia, Russia, and Australia—cooperation with reformers produced a measure of women's suffrage.<sup>44</sup>) French women missed all of these experiences. There certainly existed the same correlation with social control movements, such as anti-alcoholism (particularly in the CNFF), but this created no comparable organizations of women in nineteenth-century France, much less groupings that taught them political maneuvering or galvanized them for a suffrage campaign.

Several retardative factors contributed to the slower development of suffragism in France. Starting with de Tocqueville, attention has correctly been drawn to the powerful family orientation of Catholic societies, which were less likely to develop a woman's movement than Protestant societies in which the prevalent ethic stressed the independent action of the individual. Catholicism, furthermore, provided an institutional home for women, which had no parallel in Protestant countries: the convent and service associations gave many Catholic women an ideological identity, esteemed professions, and a sense of political participation. Such factors created a strong correlation between national religion and suffragism: virtually all early successes were in Protestant states.<sup>45</sup> French opponents of women's suffrage, particularly Radicals, stressed this difference. The traditions of Protestant democracies could accommodate women's suffrage, they argued, but in Catholic lands women would vote as their confessors dictated; in France this would mean an antirepublican reaction, undoing separation and perhaps even destroying the regime.<sup>46</sup>

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Movement in Australia," *Journal of the Royal Australian Historical Society*, 53 (1967): 299–320; N. MacKenzie, "Vida Goldstein, the Australian Suffragette," *Australian Journal of Politics and History*, 6 (1960): 194–95; Flexner, *Century of Struggle*, 184–85; Scott and Scott, *One Half the People*, 21; and Evans, *The Feminists*, 60–61. For the similar role of Butler's association in England, see Evans, *The Feminists*, 67. For further discussion of the correlation between women's suffrage and social control movements, see Alan P. Grimes, *The Puritan Ethic and Woman Suffrage* (New York, 1967), esp. ix–xii; Kraditor, *The Ideas of the Woman Suffrage Movement*, chap. 6; and O'Neill, *Everyone Was Brave*, 77–106.

<sup>44</sup> Finnish and Norwegian suffragists profited from their association with the national-constitutional struggle with Russia and Sweden respectively; Bohemian women from a similar collaboration with the liberal-national struggle within the Austrian empire; Russian women from the post-Crimean War Alexandrine reforms; Australian suffragists won the national franchise through cooperation with the federalist movement. For such comparative studies, see Evans, *The Feminists*; and Paulson, *Women's Suffrage and Prohibition*.

<sup>45</sup> Observing the situation of American women, Alexis de Tocqueville wrote, "Amongst almost all Protestant nations young women are far more the mistresses of their own actions than they are in Catholic countries. This independence is still greater in Protestant countries like England, which have retained or acquired the right of self-government; the spirit of freedom is then infused into the domestic circle by political habits and by religious opinion." Tocqueville, *Democracy in America*, 2 (2d edn., 1840; Shoken edn., New York, 1961): 237 (book 3, chap. 9). For discussion of the correlation of religion and suffragism, see Evans, *The Feminists*, 30; Grimes, *The Puritan Ethic and Woman Suffrage*, 8, *passim*; and Kraditor, *The Ideas of the Woman Suffrage Movement*, chap. 4: "Woman Suffrage and Religion."

<sup>46</sup> For his arguments about suffragism in Protestant and Catholic states, see Hélène Miropolski's interview of Clemenceau in "Le Suffrage des femmes," *Le Temps*, May 1, 1913. For a summary of the Radical argument against women's suffrage because women would vote for reaction, see Zeldin, *France, 1848–1945*, 360. For evidence revising this understanding of Radical antisuffragism, see Steven C. Hause, "The Rejection of Women's Suffrage by the French Senate in November 1922: A Statistical Analysis," *TR/TR*, 3–4 (1977): 205–37.

Social factors delaying the development of French suffragism go much further. The Roman legal tradition, vigorously reinforced in the Napoleonic Code, accentuated the family orientation. The slower pace of industrialization and urbanization, with a corresponding preservation of traditional society, helped maintain this ethic. The extraordinary demography of modern France, with a stagnant level of population growth, also contributed to maintaining this ethic; the threat of "depopulation" (particularly vis-à-vis the growth of the German enemy) produced a social norm, shared by many suffragists, that women first had a national duty to propagate the race.<sup>47</sup> Such factors make the comparison of French and English or American suffragism very difficult. In comparing suffragists in France with those in other Roman Catholic-Roman law societies, such as Italy and Spain, French women seem unusually militant even with their insistence upon legalism. Perhaps the most striking illustration of the power of such factors is Canadian: with the active help of Protestant clergy, women won the provincial franchise in all English-speaking provinces between 1916 and 1922, the right to vote in federal elections in 1918. In French Quebec, where the Catholic church under Cardinal Villeneuve vigorously opposed women's suffrage, women who could vote in national elections were denied the provincial ballot until 1940.<sup>48</sup>

The slow evolution of suffragism in France explains the preference for legalism in several ways. The pioneering feminist organizations in France (Léon Richer's LFDF and Deraismes's Amélioration) refused for a long time even to seek the vote.<sup>49</sup> The advocacy of women's suffrage was *per se* so radical an act in France that the further radicalism of extreme tactics would have drastically diminished their chances of public acceptance. The French woman who attended Clément's lecture on suffragism at Bordeaux in 1912 and thereupon joined the UFSF was as daring in her culture as was the American woman who joined Blatch in her march up Fifth Avenue that same year.

Enfranchisement was by no means the exclusive goal of feminist movements in any country. But obstacles such as the Napoleonic Code meant that French feminists faced more difficulties than their Anglo-Saxon compatriots in over-

<sup>47</sup> For another discussion of the factors making France different, see Paulson, *Women's Suffrage and Prohibition*, 43-54. For a good discussion of the whole woman question in France, dealing with such problems, see Karen M. Offen, "Aspects of the Woman Question during the Third Republic," *TR/TR*, 3-4 (1977): 1-19. Her "The 'Woman Question' as a Social Issue in Nineteenth-Century France: A Bibliographic Essay," *TR/TR*, 3-4 (1977): 238-99, is a valuable guide to work on some of these issues. Offen has kindly permitted the authors to consult her unpublished manuscript, "The Woman Question as a Social Issue in Republican France before 1914," which makes clear the importance of such French concerns as depopulation.

<sup>48</sup> Cleverdon, *The Woman Suffrage Movement in Canada*, 5, 14, 214-64. Newfoundland, not united with Canada until 1949, gave women the vote in 1925.

<sup>49</sup> For French feminism in the late nineteenth century, see Bidelman, "The Feminist Movement in France." Also see his "The Politics of French Feminism: Léon Richer and the *Ligue Française pour le droit des femmes*, 1882-1891," *Historical Reflections*, 3 (1976): 93-120, and "Maria Deraismes, Léon Richer, and the Founding of the French Feminist Movement, 1866-1878," *TR/TR*, 3-4 (1977): 20-73. For the development of feminist ideology, see Claire G. Moses, "The Evolution of Feminist Thought in France, 1829-1889" (Ph.D. dissertation, The George Washington University, 1978), University Microfilm no. 79-03787. For the 1890s, see Wyona H. Wilkins, "The Paris International Feminist Congress of 1896 and Its French Antecedents," *North Dakota Quarterly*, 43 (1975): 5-28.



coming the double standard. Since the French movement was in an earlier stage of development, more energy was naturally devoted to nonsuffrage causes. One of the first aspirations of feminists everywhere, a law to permit married women to control their own wages and property, provides a good illustration: American feminists had already campaigned for such a law for a dozen years when they won their first victory, a Married Woman's Property Law in New York in 1860. English women labored for their reform from 1855, winning minor adjustments three times before securing the Married Women's Property Act of 1882. The French campaign for a similar bill began in 1893 when Schmahl founded *Avant-Courrière*. It took fifteen years to persuade the French parliament to adopt the "loi Schmahl" of 1907. Victories such as that act and revisions of the Code concerning divorce, adultery, and guardianship persuaded French feminists that legalism was a successful strategy, the slow pace of which could best be accelerated through enfranchisement. Hence Schmahl dissolved *Avant-Courrière* in 1908, founded the UFSF in 1909, and dedicated the new organization to the patient strategy that had attended her success of 1907.<sup>50</sup>

Thus, in the years when a mass suffrage movement was beginning elsewhere, French feminists were preoccupied with campaigns others had already won or never faced; when militancy appeared elsewhere, French suffragism was just developing and still characterized by confidence in the political process. French optimists held that legalism provided an acceptable variety of tactics without unseemly behavior. Lectures, posters, pamphlets, and petitions would succeed. In this, too, the French lacked the experience of others. Auclert was seeking five thousand signatures on a petition to the Chamber of Deputies forty years after British suffragists had inundated the House of Commons with 255 different petitions bearing sixty-one thousand names. In another instance, Auclert's success in persuading the Conseil Général of the Seine to endorse women's suffrage in 1907 led the UFSF to adopt the local appeal as its chief policy (1912-14). American suffragists had learned the dubious value of such campaigns. From 1870 to 1910, they staged 480 local campaigns in thirty-three states, seeking referenda on women's suffrage; they got the issue on the ballot in only 17 cases and won 2 of them.<sup>51</sup>

French suffragists also believed that legalism was successful because they were experiencing rapid growth. In the decade 1904-14, the suffrage leagues of Paris attracted thousands of members, spread to the provincial cities, and won the backing of influential people and organizations. The UFSF was the center of this growth and its concomitant optimism. It reached one thousand members in 1910, three thousand in 1911, six thousand in 1912, ten thousand in 1913, and twelve thousand by the spring of 1914. It had chapters in nine departments in 1911, sixteen in 1912, fifty in 1913, and eighty-one in early 1914. Each year it

<sup>50</sup> See the Dossier Jeanne Schmahl and the file of her correspondence (091 SCH) at the BMD. For biographical sketches, see "Mme. J.-E. Schmahl," *L'Entente*, January 1906; and "Figures d'autrefois et d'aujourd'hui: Mme. Jeanne E. Schmahl," *La Française*, January 24, 1908.

<sup>51</sup> For such comparisons, see Auclert, *Vote des femmes, passim*; Rosen, *Rise Up Women*, 9; and Flexner, *Century of Struggle*, 222.

won the support and collaboration of organizations such as the Ligue des Droits de l'Homme, the Ligue d'Électeurs, the Ligue des Consommateurs, and the Ligue Anti-Alcoolique. Schmahl and her successors were convinced that this pattern was leading to an early victory. It is not surprising, therefore, that she did not want to "shock or alarm" actual and potential supporters. French politicians and journalists reinforced this attitude by commending suffragists for their moderation (usually compared to English women), and repeating that it would lead to the conquest of the polls.<sup>52</sup>

The relative stage of French suffragist development also encouraged moderation by permitting confidence in the Chamber of Deputies. French women had not yet experienced the discouragement of English suffragists who had seen the tabling of twenty-eight private member's bills for women's suffrage between 1870 and 1913. Suffrage bills had been introduced in the chamber in 1893 by Joseph de Gasté and in 1901 by Fernand Gautret, but they had not been debated because the French parliamentary committee system permitted a hostile *commission* to kill bills by never delivering a report on them. A bill for women's municipal suffrage introduced by Paul Dussaussoy in 1906, however, went to a Commission of Universal Suffrage, which seemed sympathetic. The president of the commission, Charles Benoist, and the *rapporteur* assigned to the bill, Ferdinand Buisson, both stated their support. Buisson presented an extremely favorable report (part of which was written by Maria Vérone) in 1909, with the unanimous backing of the committee. Simultaneously, surveys showed that the number of deputies supporting the Dussaussoy bill was increasing significantly. Such parliamentary barons as Jean Jaurès, René Viviani, Paul Deschanel, and Raymond Poincaré approved publicly. During the tenth legislature (1910-14), 238 deputies joined in supporting it; by suffragist calculations, the elections of 1914 meant 291 deputies (48.4 percent of the chamber) were committed to it. In February 1914 the bill reached the chamber's order of the day for the first time. It was a long way from becoming law, far longer than suffragists realized. Their optimism was undaunted: the 1914 poster of the UFSF proclaimed "In 1916 Women Will Vote!" When the WSPU was turning to militancy, French suffragists simply saw no need for extremism.<sup>53</sup>

This confidence of French suffragists in the parliamentary process raises a third broad area of contrast with other suffrage movements and clarifies their insistence upon moderate behavior: the relationship between suffragists and the established regime. The situation in France was strikingly different from that of

<sup>52</sup> This expansion can be traced in the Dossier UFSF, BMD, in the UFSF's *Bulletin annuel* (title varies), or in *La Française*, which was closely associated with the UFSF. But, for a strikingly different (and, in our opinion, erroneous) view of the UFSF, see Evans, *The Feminists*, 133-34.

<sup>53</sup> The Buisson report was published as a book in 1911 and provides a good introduction to the suffrage bills in the Chamber of Deputies: Ferdinand Buisson, *Le Vote des femmes* (Paris, 1911). The minutes of the Commission of Universal Suffrage, at the Archives Nationales (C 7375), and the private papers of Benoist, at the Bibliothèque de l'Institut (MS 4534 and 4535), reveal how misplaced suffragist optimism was. For suffragist calculations of deputies who backed the Dussaussoy bill, see the Dossiers Vote des Femmes, BMD. A suffragist circular letter of June 1, 1914 (Dossier Vote des Femmes, 1914, BMD) provides the names of all supporters. Also see the Dossier UFSF, BMD (including the poster quoted).

British and American women or other Continental suffragists. French suffragists were almost invariably devoted republicans. The majority supported the Radical, anticlerical government of the early twentieth century. Most were ardent Dreyfusards (as Durand and Séverine showed in *La Fronde*). Some moderate feminists, particularly in the CNFF, were more comfortable with the conservative republicanism of the *progressistes*, but all shared the vision of a laic, reformist republic that protected private property. The role of women in the Commune, which Richer and Deraismes had denounced, held no appeal for suffragists, despite the case of Eliska Vincent. Equally few shared the anti-republican sentiments of Boulangism, despite the early connection of Durand and d'Uzès with it. French suffragists, in short, considered themselves part of the established regime; they merely sought a direct role in it.

This attachment to the Third Republic was not solely ideological; it was intertwined with the personal lives of French suffragists. Louise Cruppi, for example, was the head of one of the six administrative sections of the CNFF. She was also a Crémieux—born to the aristocracy of radicalism. Her husband was a deputy from the Haute-Garonne, Jean Cruppi, who served as Clemenceau's minister of commerce, Monis's minister of foreign affairs, and Caillaux's minister of justice. The Cruppis were not a unique couple. Mme. Pichon-Landry, an officer of both the CNFF and the UFSF, was the wife of Stephen Pichon, senator from the Jura and minister of foreign affairs in five cabinets. Julie Siegfried, the president of the CNFF, was the wife of Jules Siegfried, a republican deputy from Le Havre and *doyen* of the chamber at the end of the war. Jeanne Chauvin was the sister of Émile Chauvin, a Radical-Socialist deputy from Seine-et-Marne. Valentine Thompson, the editor of *La Vie féminine* and a leader of the UFSF, was the daughter of Gaston Thompson, who sat in six cabinets. A president of *Études* married Maurice Faure, Briand's minister of public instruction. Marie Georges-Martin, the head of the suffrage section of the CNFF, married a Radical senator from the Seine. Marguerite de Witt-Schlumberger, a president of the UFSF, was the daughter of a deputy from Calvados (as well as being the granddaughter of Guizot). Alice Mollard, a member of the executive bureau of the Union Fraternelle des Femmes, was the daughter of Antoine Mollard, a deputy and senator from the Jura. Mme. Pierre Goujon was the daughter of Joseph Reinach, Gambetta's secretary and a prominent Dreyfusard deputy. It is hardly surprising that one leader of the CNFF proclaimed that French feminist organizations were "essentially republican institutions."<sup>54</sup>

These political connections do not seem, *prima facie*, to distinguish French suffragists from others. A survey of eighty-nine prominent American suffragists, for example, has shown that more than half of the women came from families in which the men were active in politics. The 168 "suffrage prisoners" of the Woman's Party jailed by the Wilson administration included daughters of congressmen from Connecticut and Kentucky, daughters of a state senator and a

<sup>54</sup> Avril de Sainte-Croix, *Le Féminisme*, 175.



Figure 3: French suffragists laying flowers at the statue of Condorcet after the parade of July 5, 1914. Photograph reproduced courtesy of the Bibliothèque Marguerite Durand, Paris.

secretary of state, and the niece of Vice President Stevenson. Similar ties to the governing elite existed in other countries.<sup>55</sup>

Suffragists in English-speaking countries, however, had a fundamentally different relationship with their government. They could agitate in public without considering that their actions might threaten the existence of the regime to which they were bound. The chance that the American republic might collapse, or the English monarchy fall, due to suffragist demonstrations was so small that it did not shape women's behavior. In contrast, the republicanism of French suffragists sensitized them to the apparent instability of the newly founded republic, whose two predecessors survived for a total of ten years. The formative years of French suffragism coincided with the *seize mai* crisis, Boulangism, the Panama scandal, the Dreyfus affair, and the battle over separating church and state. Republicans whose political beliefs matured during that generation instinctively hesitated in their agitation for reform, fearing to precipitate the crisis that might

<sup>55</sup> Scott and Scott, *One Half the People*, 165. Brief biographies of the American suffrage prisoners are given in Stevens, *Jailed For Freedom*, App. 4: pp. 354–71. A similar pattern existed in New Zealand, for example, where the leader of WCTU suffragism, Kate Sheppard, was the wife of a Christchurch councillor, and the executive committee of the Women's Franchise League included several wives of politicians; Grimshaw, *Women's Suffrage in New Zealand*, 36, 50.

topple the republic. Hence, republicanism made French suffragists innately reluctant to stage demonstrations in the streets. When they dared to do so, as in the *manifestation Condorcet*, they dutifully applied to the Préfecture of Police and the Ministry of the Interior for permission.<sup>56</sup>

Suffragist behavior during World War I illustrates the power of this concern. French suffragists saw the republic as once more in danger and ceased even the most timid propaganda.<sup>57</sup> When the Pankhursts were suddenly confronted with a threat to the kingdom, they too abandoned militancy. American suffragists, untroubled by any perception of such dangers, continued their militant demonstrations. The vigorous campaign of the Woman's party, begun in the Washington demonstrations of 1913, actually reached its peak during the war, with over five hundred arrests between June 1917 and the spring of 1919.

The attachment of suffragists to the French republic distinguished them from other Continental movements as well. The feminist movement in Germany and Russia did not operate under a liberal-democratic government based on the representative principle and the sovereignty of parliament. Suffragists, drawn from the educated middle classes, were uncomfortable with the established regime. Their closest allies were generally left-wing liberals (the Kadets in Russia and the Liberal People's party in Germany), ideologically comparable to the Radicals in France but utterly lacking their power. Suffragists could not rely on the government; indeed, they could not expect to succeed without significant changes in the constitutional structure. Simultaneously, however, they were unwilling to oppose the regime forcefully, fearing to advance the socialist revolution that would destroy the bourgeois society they wished to represent. Thus, moderation in many Continental countries was in fact a "tactical paralysis" of people who could neither fully support nor challenge the regime.<sup>58</sup>

Several French cabinets recognized the affinity of suffragists for the republic and made concessions toward accepting women in politics. Women were gradually permitted to vote and to serve in their professional *conseils* and *conseils supérieurs*, winning the vote for the *Conseils des prud'hommes* (trade arbitrators), for example, in 1907. René Waldeck-Rousseau, whose choice of a socialist as his minister of commerce in 1899 marked a historic departure in French politics, deserves similar note for naming Bonneval to the *Conseil Supérieur du Travail* in 1900. Even Émile Combes, an unrelenting foe of women's suffrage, named Avril de Sainte-Croix to the extra-parliamentary Commission du Régime des Mœurs

<sup>56</sup> For suffragist negotiations with the police, see Camille Bélilon to Arria Ly, May 7, 1914, BHVP; police report, June 9, 1914, Dossier Vote des Femmes, APP, B<sup>A</sup> 1651; Ministry of the Interior to the Préfecture of Police, June 16, 1914, Dossier Manifestations, *ibid.*; Undersecretary of State for Beaux-Arts to Séverine, July 1, 1914, Dossier Manifestation Condorcet, BMD; and "Manifestation du 5 juillet," *La Française*, June 27, 1914.

<sup>57</sup> For the impact of World War I on French suffragism, see Steven C. Hause, "Women Who Rallied to the Tricolor: The Effects of World War I on the French Women's Suffrage Movement," *Proceedings of the Western Society for French History*, 6 (1979): 371-81. For a broader study, see James F. McMillan, "The Effects of the First World War on the Social Condition of Women in France" (D.Phil. thesis, Oxford, 1976).

<sup>58</sup> The term "tactical paralysis" is from Evans, *The Feminist Movement in Germany*, 108. For his discussion of the relationship with the government, see *ibid.*, 8-23; and, for relations with liberal parties, see *ibid.*, 84-86. For the connection between Russian suffragism and the liberals of 1905, see Stites, *The Women's Liberation Movement in Russia*, 198-209, esp. 199, 204.



in 1904 and six women (five of whom were active suffragists) to the twenty-six member Comité de Réforme du Mariage in 1905. This trend, especially marked under feminist ministers such as Viviani (a member of the LFDF from the 1880s), culminated in 1936 when Léon Blum named three women under-secretaries (including two long-time suffragists) to the Popular Front cabinet.<sup>59</sup>

The relationship of French suffragists to the republic was defined by the stick as well as the carrot. When WSPU militancy was beginning in 1906–09, the Clemenceau government was reminding French women of the repressive powers of the state by its treatment of labor unrest. Clemenceau enjoyed referring to himself as “the number one cop [*flic*] in France.”<sup>60</sup> Could feminists doubt him? In 1906 he mobilized twenty thousand soldiers against the striking coal miners of Courrières-Lens; in 1907 he turned the army loose on demonstrating wine growers in the Midi, with results such as those at Montpellier where troops fired on the crowd, killing several protesters; and in 1908 he arrested labor leaders during another strike and again called out troops who fired on a crowd at Villeneuve-Saint-Georges.<sup>61</sup> The army shot demonstrators without regard to their sex, a fact that confronted French suffragists precisely in the years when Auclert, Pelletier, and others proposed aggressive tactics but found no followers.

French suffragists possessed unmistakable evidence about Clemenceau's sympathies for their movement. In 1907, he published a pamphlet entitled *La “Justice” du sexe fort*, which contained the assertion, “If the right to vote were given to women tomorrow, France would all of a sudden jump backwards into the Middle Ages.”<sup>62</sup> Clemenceau's socialist minister of labor accepted troops firing on strikers; would feminist sympathizers in the cabinet have hesitated to authorize force against violent suffragettes? The Préfecture of Police and the Ministry of the Interior were ready. Their agents had already infiltrated suffragist organizations and recorded their plans. Large numbers of policemen awaited all feminist demonstrations. When Pelletier addressed a rally at the Carrefour Feydeau in 1914, reporters found policemen outnumbering suffragists. Files were kept on feminist aspirations and on the tactics of the English suffragettes. Surveillance of French women meeting with Christabel Pankhurst was maintained throughout her exile in Paris. Even small demonstrations were sometimes met with force. When Auclert led the march of 1904 to the Vendôme Column, her followers did not witness the planned burning of the Civil Code; instead, they saw Auclert brusquely manhandled by a cordon of police who expected her.<sup>63</sup> The French

<sup>59</sup> An indication of the importance that French suffragists attached to this recognition of their devotion to the republic may be seen in Avril de Sainte-Croix, *Le Féminisme*, 164–69.

<sup>60</sup> Georges Wormser, *La République de Clemenceau* (Paris, 1961), 206.

<sup>61</sup> For a detailed illustration of this policy of order, see Jacques Julliard, *Clemenceau, briseur des grèves: L’Affaire de Draveil Villeneuve-St. Georges, 1908* (Paris, 1965).

<sup>62</sup> Georges Clemenceau, *La “Justice” du sexe fort* (Paris, 1907), 30.

<sup>63</sup> Most of the files of the Police Générale at the Archives Nationales (série F<sup>1A</sup>) provide no help on the suffrage question, but there is a valuable series of police dossiers (F<sup>7</sup> 13266) in the Ministry of the Interior papers. This includes files on the English suffragettes, activities of Parisian feminist groups, and feminist *revindications*. The Préfecture of Police kept several files on feminists, notably B<sup>A</sup> 1651, which included dossiers on suffrage demonstrations and feminist groups. They also kept dossiers on individuals, notably Auclert (B<sup>A</sup> 885) and Séverine (B<sup>A</sup> 1660). The police records in some Archives Départementales (Série 4 MP) include similar files. The records of the department of Seine-Maritime at Rouen, for example, include police files on feminist activities



Figure 4: A contemporary artist's depiction of French suffragists disrupting the voting during the municipal elections of May 1908 in Paris. Illustration reproduced courtesy of the Mary Evans Photo Library.

were certainly not alone in their willingness to repress suffragist militancy, as the women confined to Holloway Prison for demonstrating in London or to Occoquan Workhouse for protesting in Washington, D.C., could testify. French governments, however, had a record of more extreme repression. British and Ameri-

(4 MP 4670) dating back to speeches by Deraismes and Rouzade in 1880. For the police and Place Vendôme demonstration, see Hubertine Auclert, "Le Féminisme," *Le Radical*, November 5, 1904; and reports of the police infiltrator in Suffrage des Femmes, Dossier Hubertine Auclert, APP, B<sup>A</sup> 885. For police at the Carrefour Feydeau demonstration, see Parisian newspapers for March 30, 1914 and Dossier Vote des femmes, 1914, BMD. Evans has also stressed the "repressive machinery of the state"; *The Feminists*, 194.

can protesters might face the terrible prospect of force-feeding; the incident at Montpellier showed French feminists that greater force could be used.

The only instance of suffragist violence in France was spontaneous and brief, so it produced no such tragedy. Yet it still taught suffragists that only moderation would work. On the eve of the municipal elections of 1908, Auclert had speculated in her newspaper column on the need for direct action. "For women to obtain the vote in France," she asked, "are they going to be obliged to knock down the urns that contain only male votes?"<sup>64</sup> A few days later, she led a handful of demonstrators to disrupt the balloting at several voting places. Late in the day, Auclert burst into the *mairie* of the Fourth Arrondissement, seized the voting urn, smashed it to the ground, and trampled on the ballots in front of the scandalized poll workers. One week later, during the run-off elections, Pelletier also resorted to violence. When denied entrance to a poll in the Fifth Arrondissement, she threw stones through the window and was arrested.

These actions did not inaugurate a suffragette campaign in France, nor produce a single protest parade. Instead, French suffragists overwhelmingly repudiated them. *La Française* rushed to lambast such behavior: "We are absolutely opposed to . . . violent public demonstration." The entire movement hastened to proclaim legalism the only acceptable policy. One month after the municipal elections, the Congrès National des Droits Civils et du Suffrage des Femmes repudiated the actions of Auclert and Pelletier. French militants thus faced a simple choice: adhere to legalism or accept ostracism. Auclert, always a pragmatist, chose to recant. In a rousing speech to the congress, she admitted that her act was a failure because too few women supported her. Whereupon she proposed a return to legal tactics and received a standing ovation from the delegates. Auclert died in 1914, convinced that militancy "would not have any success" in France and that women would "arrive at our destination by persuasion."<sup>65</sup>

AFTER A LIFE OF MILITANCY, Auclert recognized that the movement she had founded would not follow her example. The forces circumscribing suffragist behavior in France were too powerful. Briefly summarized, none was stronger than the bourgeois nature of the movement, with a shared dedication to respectability, dignity, and the avoidance of ridicule. The effect of these middle-class values was especially strong in France, where the campaign did not receive the leavening of working-class experience that it did in Britain; the class struggle was sufficiently acute to prevent socialist-suffragist cooperation or the suffragist adoption of tactics associated with socialism, such as outdoor demonstrations. Furthermore, the stage of feminist organization meant that the era of women's

<sup>64</sup> Hubertine Auclert, "Le Féminisme," *Le Radical*, May 2, 1908.

<sup>65</sup> "Les Suffragistes anglaises," *La Française*, June 28, 1908; "Après l'assemblée des femmes: Les Impressions de Marguerite Durand," *La Liberté*, July 1, 1908; Dossier Congrès National des Droits Civils et du Suffrage des Femmes, BMD, especially Eliska Vincent to Marguerite Durand, May 18, 1908; Jeanne Oddo-Deflou, ed., *Congrès national des droits civils et du suffrage des femmes, tenu . . . les 26-28 juin 1908: Compte rendu in extenso . . .* (Paris, 1910), 216-17, and "Le Congrès national des droits civils et du suffrage des femmes," *La Liberté d'opinion*, May-June 1908, 120; Auclert, as quoted in "L'Action directe et les plus notoires féministes françaises," *Gil Blas*, February 26, 1913; and Dossier Vote des Femmes, 1910-13, BMD.



*Figure 5: Maria Vérone (1874–1938), suffragist leader of the Ligue Française pour le Droit des Femmes. Photograph reproduced courtesy of the Bibliothèque Marguerite Durand, Paris.*

militancy elsewhere coincided with a period of optimism in France. French suffragists believed that legalism would work: it was securing other feminist reforms, the Dussaussoy bill seemed to be advancing in the Chamber of Deputies, and the rapid growth of the suffrage movement suggested that public opinion was being conquered. If a final push were needed, the tactics of legalism offered

sufficient means to success. Finally, French suffragists had a unique relationship with their government, creating a situation different from both Anglo-American and other Continental suffrage campaigns. They were deeply committed, both ideologically and personally, to a regime that appeared to be unstable. They could not assume the survival of liberal democracy, as English-speaking suffragists did, but they could expect more cooperation from the government than other European suffragists. They chose activities that did not threaten the bourgeois republic, knowing that if they acted otherwise it might protect itself with a ferocity exceeding the use of force against other suffrage campaigns.

Perhaps no one better illustrates French attitudes to militancy than Maria Vérone. A militant, she spent several nights in jail for testing the limits of legalism, and she certainly contemplated the need for vigorous demonstrations. Yet she never lost her optimistic conciliation or dedication to the republic and its laws. Vérone sat in the gallery of the Senate in November 1922 and watched as several senators switched the votes they had cast for women's suffrage as deputies when the suffrage bill passed in 1919. When the project to which she had devoted much of her life was beaten, Vérone stood up and shouted to the Senate. Did she summon the myrmidons to a campaign of suffragette demonstrations at long last? No. Her words were, "Long live the Republic, all the same!"<sup>66</sup>

<sup>66</sup> Suzanne Grinberg, *Historique du mouvement suffragiste depuis 1848* (Paris, 1926), 175; "Le Sénat refuse le vote aux femmes," *La Française*, November 25, 1922; and Dossier Maria Vérone, BMD.



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## Reviews of Books

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### GENERAL

MICHAEL KAMMEN, editor. *The Past Before Us: Contemporary Historical Writing in the United States*. Ithaca: Cornell University Press, for the American Historical Association. 1980. Pp. 524. \$19.95.

Sixteen years ago, in concluding a historical survey of professional historical writing in the United States, I opined that current tendencies were decidedly upward. In quality as well as quantity, historians and historical scholarship were doing well. Ten years ago the editor of the next major stock-taking, "Historical Studies Today" (*Daedalus*, Winter 1971), went a step further by declaring that "professional historical investigation is flourishing and is as robust as it has ever been" (p. v). Now, in a similar work executed on a more comprehensive scale than either of these predecessors, editor Michael Kammen tells us that the historical profession in the United States "is undergoing the most creative ferment in its entire lifetime" (p. 22). These generalizations form an interesting trend that, if it could be extrapolated into a law, might state that the cheerfulness of the historiographer increases with every examination of the health of the profession.

Whatever the truth of these sanguine judgments (a question to which I shall return), their similarity betrays a common origin. In all three cases the historiographers have been well-established scholars commenting on activities in which they, their friends, associates, and rivals have been intimately involved. Notwithstanding an occasional gesture toward a wider audience, all three works are really addressed to fellow historians and to graduate students. All three, moreover, take note of a disturbing contrast between the proud achievements of professional scholarship and the much less satisfactory condition of history outside this professional circle—in the classroom or in the literary culture, for example—and all three offer their readers some assurance that the problem (variously defined) may not be insoluble. In the light of these facts, our historiographical optimism looks like a way of sustaining the spirit and strengthening the solidarity of the

group in an increasingly problematic if not inhospitable environment. Although our historiography is overtly self-critical, at a deeper level does it not function as an elaborate form of boundary maintenance?

I suspect it does, and not only vis-à-vis the world outside our professional networks. Historiography also articulates the self-consciousness of successive generations of scholars. It enables a newly ascendant generation to define itself by distinguishing its own efforts from those of its predecessors. Kammen draws the generational boundary quite explicitly by beginning his lively introduction with a roll call of the senior historians who passed from the scene in the 1970s and whose departure left the leadership of the profession in younger hands. The rest of the introduction identifies what seems new in the historical scholarship of the last decade. Generational boundaries emerge even more sharply in a fine chapter on U.S. historians of Africa, wherein Philip Curtin shows how their clustering within a narrow age range explains their remarkable productivity in the 1970s and also raises some troubling questions about the future of the field.

Yes, there are troubling questions about the future in *The Past Before Us*. The glad tidings in these pages have to do mostly with the immediate past. The 1970s, as Kammen points out, were a decade in which initiatives that historians had earlier advocated and tentatively launched were systematically pursued. They were a harvest time in urban history, in quantitative studies, in ethnic history, in family and women's history; but it appears not to have been much of a seed time for new agendas, new ways of thinking, new paradigms. Family history, as sketched by Carl Degler, stands out as one exception. Diplomatic history may be another; Charles S. Maier, who is more severe than any other contributor about recent achievements in his field, articulates an exciting perspective for revitalizing the history of international relations. But most of the authors of this volume describe fields of study in which integrative concepts seem less powerful than they once did, with the result that specialization is becoming less manageable. The point is sometimes made explicitly: in Peter Stearns's

comment on the splintering of social history into more and more subfields, each with its own chronology; in David Brody's warning that the continued vitality of the historiography of the American worker now depends on construction of a "usable framework" in which to place the abundant new findings; in a final, somber chapter by Hazel Hertzberg on the teaching of history, which concludes that our classrooms also need unifying ideas more than anything else. This recurrent, submerged concern over fragmentation leaps to the fore in William H. McNeill's searching examination of modern European history. Here the academic study of European history is presented as "a powerful fountain whose jet ascended high into the air, only to break up into unstable, inchoate shapes, dispersing in every direction before commencing a glorious descent. The cascade of a descending fountain is entrancing to the beholder—far more spectacular, in fact, than the narrowly focused, ascending jet. But the fountain can persist only if an ascending jet sustains descending multiplicity. It seems to me, for one, that the jet has been turned off" (p. 109).

A reading of the dark side of this generally celebratory book may find further confirmation in the chapters on quantification and comparative history. These are, it is fair to say, the research strategies from which the greatest advances have been anticipated during the last two decades. J. Morgan Kousser describes the increasing sophistication of the most devoted historical quantifiers. Yet his essay also yields evidence that the growth of quantification reached a plateau in the 1970s and that a casual, eclectic approach to numbers is likely to remain typical of historians. In contrast to Kousser's advocacy, George M. Fredrickson writes about comparative history with refreshing detachment. His survey of results to date shows that comparative studies, strictly defined as investigations of two or more noncontiguous societies, are still sparse and scattered. A looser definition would have permitted a more affirmative judgment; a comparative consciousness has spread far and wide among historians in recent years. (The same can be said of a numerical consciousness too.) Nevertheless the Fredrickson and Kousser essays make clear that neither the comparative nor the quantitative method is having the triumphant and transforming success its earlier proponents envisaged. Neither is holding together the soaring jet of historical scholarship or revealing a rainbow in the glistening spray.

So *The Past Before Us* supplies a critical reader with grounds for distrusting its own happy thesis, and that is not the least of its merits. The great strength of the book, however, lies in its rich, extraordinarily varied substance. There are wonderfully informative chapters on what Americans have done about large areas of history, not only modern

Europe and Africa but also medieval Europe (Karl F. Morrison), early modern Europe (William J. Bouwsma), Latin America (Charles Gibson), the Middle East (Nikki R. Keddie), and southern and eastern Asia (John Whitney Hall)—this last an especially bountiful synthesis. There are chapters on methods, including oral history by Herbert T. Hoover, and an admirably specific account of the progress of psychohistory, in which Peter Loewenberg shows that this beleaguered enterprise is still a going concern.

At the heart of the book are eight topical chapters about aspects of American and modern European history. Some of these chapters—Stearns on social history, Robert Darnton on intellectual and cultural history, and Maier on international relations—deal with the work of Europeanists in the United States as well as Americanists. They help us to see what common themes engage these still distinct intellectual communities; but one also gets the impression that neither group, sadly, has much direct influence on the other. The remaining five chapters (on political history, labor history, local and urban history, Negro history, and women's history) focus entirely on American experience. Here I found especially rewarding the deft clarity of Allan G. Bogue's report on the "new" political history and the dauntless range of Kathleen Conzen's treatment of American local history. Her essay strikingly demonstrates how nearly all the strands of the "new" social history have led researchers into the study of particular local communities. Almost without exception these and the other chapters are abundantly footnoted and can therefore serve as bibliographical guides as well as general assessments.

What is left out? An assigned chapter on the "new" economic history did not materialize. There is nothing on military history or legal and constitutional history, almost nothing on the history of religion in the modern period, little on the history of science, and only passing references to the history of education. Large parts of American ethnic history go unnoticed. The histories of specific American events, periods, and regions are touched on very lightly. These are substantial lacunae, but they seem slight in comparison to what the book encompasses.

Another omission is more significant because it tells us something important about the present state of the profession. We have here neither a treatment of the philosophy of history nor a serious analysis of such theoretical issues as Marxism, idealism, structuralism, and the like. (For example, the chapter on labor history does not mention Marxism.) In the *Daedalus* volumes on contemporary historiography a decade ago, theoretical questions concerning the nature of historical knowledge and causation played a small but nonetheless vital role.

Those questions occupied a major section of my history of American historiography up to the early 1960s. That the larger questions of historical theory have dwindled from a commanding to a virtually nonexistent place in successive historiographical works reveals a real change in the intellectual life of our profession. Leading historians used to debate passionately the big, speculative issues of historical thinking. Every annual convention had at least one overflowing session in which the air crackled with controversy over a fundamental issue of general interest. Then, gradually, the ideological fervor drained out of these encounters. Positions were refined, issues compromised, and the virtues of an eclectic pluralism more and more espoused. *The Past Before Us* reflects the deadness of today's historical profession to grand theory. The "creative ferment" Kammen acclaims has sprung up while the fires of controversy were going out.

Is it possible that the two phenomena—a decline in theory and an improvement in practice—may be actually and integrally connected? Scholarship often becomes more impressive as sweeping theories and crudely formulated controversies are transcended. That is surely part of what has happened: a process of refinement and complication, turning vision into substance and making the growth of historiographical optimism over the last two decades not simply a morale-building expression of group solidarity but also a measure of accomplishment. The virtuosity with which Kammen and his collaborators have summed up the latest stage in the process is itself an authentication of today's capabilities. Yet McNeill's image of a fountain suggests a less comforting view of relations between theory and practice in modern American historiography. A discipline that uses up its big ideas invites exhaustion.

I believe that the entire period from the 1900s to the present is best understood as a single historiographical era dominated by one continuing program. The program aimed to extend the horizons of history by linking every segment of society and every level of human interaction within coherent, meaningful patterns. At first this grand program was a vision—at once democratic, humane, cosmopolitan, and vague. It inspired intense opposition as well as immense enthusiasm. It also generated sharp divisions among its supporters as they probed the possibilities of the program with a multitude of philosophical questions and ideological commitments. In time external opposition collapsed. The ideological, visionary, speculative drives of the program's supporters flagged too, not because a pure-and-simple professionalism was irresistible but because idealistic impulses lost strength in the country at large. More and more of the energy enlisted in the program shifted from broad theoretical concerns to the exercise of craftsmanship; and the accu-

mulation of professional finesse made the transfer abundantly productive.

Our problem now is to discover what new theoretical energies the fountain of historical scholarship can draw upon. The traditional vision of pushing the boundaries of inquiry outward in space and downward in society has apparently reached its limits. Further extension of the horizons of history no longer seems credible. Does this mean that an era in historiography has ended and that we need a new program suitable to an age of limits? Or can the existing program be refocused? These might be questions for the next symposium on historiography to take up.

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BURLEIGH TAYLOR WILKINS. *Has History Any Meaning? A Critique of Popper's Philosophy of History*. Ithaca: Cornell University Press. 1978. Pp. 251. \$18.50.

Philosophy of history has always been a field separate from history proper. It has always been cultivated by philosophers and should be distinguished from historiography (in the sense of historical methodology) or historical theory, which have both been worked from inside the discipline by historians proper, especially of late. Philosophy of history—which deals with the relationship between historical reality and other kinds of reality and of historical method with other kinds of method—has found respectability among historians for two reasons. First, speculative philosophers of history like Grotius, Voltaire, Hegel, and Marx have been general intellectuals who have also been historians; second, some recent philosophers of history, while belonging to the philosophical guild, have written good history. They include Dilthey, Cassirer, Croce, Collingwood, Morton White, and Morris Mandelbaum.

These considerations have been raised because *Has History Any Meaning?* is a book on the philosophy of history written by a philosopher for other philosophers. Burleigh Taylor Wilkins, a professor of philosophy at the University of California at Santa Barbara, has delivered a critique of Popper from within Popper. He chooses not to put Popper and his ideas about history in their historical context, although his throwaway lines show that Wilkins knows this context well. He takes the structure of his argument from Popper's final chapter in *The Open Society and Its Enemies* (Wilkins's title comes from this book); and, although he does adduce arguments from *The Poverty of Philosophy*, he puts them in a philosophical rather than a historical context. First, he accepts Popper's definition of historicism, which is a sport to historians. Second, he uses

the philosophical statements of nonhistorians like Walsh, Donegan, Davidson, Hempel, Dray, Danto, Oakeshott, Collingwood, Passmore, and Watkins to correct and explain Popper. Third, since Wilkins recognizes that Popper is "a systematic philosopher" who develops "a unified point of view" (p. 218), he feels justified in treating Popper's anti-inductionist view of scientific method and Popper's general ethics as automatically related to the philosophy of history.

Wilkins's own arguments are festooned with "I think" and "I believe." This habit is justified not only because Wilkins is a courteous scholar but also because he operates within Popper's framework. Thus he elaborates on Popper as much as he criticizes him. Although he accepts Popper's apparent animus against historicists (in his sense), Popper's championship of what Dray has happily denominated "the covering law model of explanation" finds Wilkins's approval. Indeed, if there is a tendency in Wilkins's criticism, it is against Popper's caution in his philosophy of history. Wilkins argues against Popper's imputation of triviality in the universal laws used by historians and against Popper's imputation of untestability to historical theories. Wilkins argues for Popper's admission of a sense in which history does have a total meaning. Wilkins asserts, finally, that he is pro-induction (as opposed to Popper's anti-inductionism) in the general philosophy of science.

Historians, finally, will oppose Wilkins's conception in turning this "critique" of Popper into a book; they may doubt that "Popper is as important to contemporary philosophy of history as Hegel was to that of the nineteenth century" (p. 13). What they have in common is only that Wilkins has written books on both.

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ILSE N. BULHOF. *Wilhelm Dilthey: A Hermeneutic Approach to the Study of History and Culture*. (Martinus Nijhoff Philosophy Library, number 2.) Boston: Martinus Nijhoff. 1980. Pp. 233. \$37.00.

H. P. RICKMAN. *Wilhelm Dilthey: Pioneer of the Human Studies*. Berkeley and Los Angeles: University of California Press. 1979. Pp. viii, 197. \$14.95.

The appearance of these two books, following closely upon Rudolf A. Makkreel's *Dilthey: Philosopher of the Human Studies* (1975), Michael Ermarth's *Wilhelm Dilthey: The Critique of Historical Reason* (1978), and other recent studies, attests to a growing if belated interest among Anglo-American historians, philosophers, social scientists, and literary

scholars in one of the least-known but most original and significant figures in the rich intellectual life of late nineteenth-century Germany. Dilthey has remained relatively obscure partly because of the resistance, until recently, of Anglo-American intellectual life to continental philosophy; partly because only fragments of his large and wide-ranging *oeuvre* have been translated thus far, including Rickman's own *Wilhelm Dilthey, Selected Writings* (1976); partly because, even in the original German, Dilthey's writing is dense and difficult; and partly because no single work by Dilthey adequately summarizes or represents his thought as a whole. The recent revival of interest in Dilthey, as both H. P. Rickman and Ilse N. Bulhof take pains to show, is due partly to a mounting revolt pioneered by Dilthey and others against positivism and historicism, which dominated Anglo-American thought for so long; and partly to Dilthey's prefiguration of several intellectual movements that came to prominence in the 1960s and 1970s—notably phenomenology, the Frankfurt school, structuralism, semiotics, and hermeneutics.

These two points are not unrelated; Rickman and Bulhof both take as their starting point Dilthey's critique of positivism and historicism (which is only positivism applied to history), which in turn laid the groundwork for his theory of the human studies (*Geisteswissenschaften*) as distinct from the natural sciences (*Naturwissenschaften*). Both authors identify a common set of problems first addressed by Dilthey and subsequently by Weber, Husserl, Heidegger, Lévi-Strauss, Habermas, Gadamer, and others: the meaning of meaning, the meaning of the past, man's historicity, the validity of interpretation, the universal within the particular, and the relationship of theory to practice.

Rickman's work is less an intellectual biography in the usual sense than an analysis and explication of such characteristic Diltheyan concepts as *Lebensphilosophie* (the philosophy of life in its totality as the proper subject of historical inquiry); *Verstehen* (the special form of historical understanding that calls for participation in, and not just knowledge of, the past); *Erlebnis* (experience as the ground of historical understanding, as that which affects us personally and is not just part of an impersonal flow of events); Dilthey's conception and classification of world-views; and his descriptive psychology. Of special interest is Rickman's discussion, at the beginning and end of the book, of Dilthey's fruitful approach not only to the questions of what has been, what is, what is likely to be, and what is possible but also to the question of what is desirable, which looms ever larger in the most vigorous intellectual currents of our time.

Bulhof's more ambitious study deals with Dilthey primarily, but not exclusively, as a pioneer of the



hermeneutic approach, which views "history as a literary text being written by mankind—a text in which the author himself, mankind—figures as the hero" (p. 8). Dilthey recognized that all historical interpretations, including his own, are inevitably influenced by the historian's own experiences and the culture and times in which one lives. Instead of denying or ignoring these influences, however, Dilthey made them the starting point of his "hermeneutic circle," a concept he borrowed from Schleiermacher to describe a technique of textual interpretation "by which the alien world of the other is transmuted into a world the interpreter and his readers can understand and appropriate" (p. 58). This approach involves three interdependent operations: relating words to the totality of the text, relating the text as a whole to the mentality of its author, and relating the text to the literary genre to which it belongs. Bulhof persuasively shows that the ultimate aim of Dilthey's hermeneutics, which pervades his entire system and proved most fruitful for the future, is to enable us to relate the known part (the text itself) to the larger—at first unknown and never completely knowable—context that gives the parts their meaning. In contrast to historicism, with its concept of time as an irreversible process which has the effect of sterilizing the past and alienating us from it, the past of hermeneutics retains its validity and can touch our lives existentially.

Both books are welcome additions to the growing literature on Dilthey. But what is missing from both—indeed, from most recent studies of Dilthey—is a critical assessment of his achievement. Neither Rickman nor Bulhof comes to grips with Carlo Antoni's charges, for example, that Dilthey's philosophy of history fails to account for change and becoming, that his concept of world view (*Weltanschauung*) is abstract and reductionist, and that he failed to assign a place in his system to rational thought between the abstract world view and the precognitive life experience or with Lukács's charges that Dilthey's system was hopelessly subjective, irrationalist, led to gross falsifications of the real past, and provided an ideological apology for the reactionary Second Reich. Let us hope that forthcoming studies will address these issues.

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YIRMIAHU YOVEL. *Kant and the Philosophy of History*. Princeton: Princeton University Press. 1980. Pp. xv, 325. \$18.50.

This book deals with Kant's concept of history and its place in his philosophical system as a whole. Drawing with impressive scholarship upon the whole corpus of Kantian writings, Yirmiahu Yovel

argues that the importance of that concept for the system has been much undervalued: that history as the process in which freedom and nature—the two great opposites of Kantian thought—are somehow to be "synthesized" and in which "the highest good" is to be realized is absolutely essential for its completion. At the same time Yovel concedes that this synthesis is not achieved—indeed that it never could be achieved within the Kantian framework, lacking as it did the concept of dialectic that allowed Hegel later to adumbrate a "history of reason" in which the concept of reason is itself historicized. Yovel's argument is more concerned with the problem of constructing systems of philosophy than with how to interpret real or "empirical" history. (Kant's goal is characterized as the working out of a "critical" philosophy of history, but this, of course, means "critical" in the sense of the three great *Critiques*, not that made familiar by contemporary Anglo-American critical philosophers of history.) No revisionist account of Kant's view of history can do much about the fact that Kant, unlike Hegel, was really not very interested in "what actually happened." Once having pointed to the Enlightenment as the great turning point of world history conceived as the history of reason, his interests were all forward looking: what concerned him was the idea of future history as a moral task. This makes relevant enough Yovel's long discussions of Kant's concept of the highest good, his moral proof for the existence of God, his problem of the relation between natural and moral teleology, and much more. There is little here, however, of direct interest either to historians or to theorists of historiography.

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FREDERICK A. OLAFSON. *The Dialectic of Action: A Philosophical Interpretation of History and the Humanities*. Chicago: University of Chicago Press. 1979. Pp. ix, 294. \$22.00.

Beginning with the work of Patrick Gardiner and William Dray in the 1950s, Anglo-American philosophers have examined aspects of historical understanding with impressive rigor and precision. But their focus has been rather narrow, largely determined by the terms of the covering law debate. On the whole, historians have found in analytical philosophy no more than a partial account of historical practice. In a well-argued book that breaks new ground, Frederick A. Olafson develops a more adequate analysis of the way history actually goes about its business.

Olafson's originality consists in combining a logical and conceptual orientation, exemplified by Dray and Georg von Wright, with a phenomeno-



logical analysis of the object of historical inquiry. In his third chapter he outlines a conception of "the historical process and of 'historicity' as a dimension of human existence that is centrally implicated in that process." This provides a solid basis for the examination of problems of historical explanation that follows. Both these chapters, in turn, rest on a discussion of the broader question of the nature of man and the ways in which he can be known. Olafson locates history within the humanities and suggests that the adjective "humanistic" offers a clue to an aspect of history that is slighted by theories of explanation that take the procedures of the natural sciences as their model. His thesis is "that literature and history share a heavy commitment to the narrative mode as the following of human action through time and that the distinctive character of the narrative order they exhibit must be understood by reference to the intentional character of the process these actions compose."

Although Olafson's emphasis on the narrative mode, teleological explanation, and links with literature is very much to the point, a historian may still be inclined to maintain that history is *sui generis* and that its distinctiveness is constituted, in part, by affinities with both the social sciences and the humanities. Its approach is diachronic and synchronic, idiographic and nomothetic. In defining the distinctive character of historical understanding, especially its relationship to the social sciences, the insights of analytical philosophy and phenomenology have to be complemented by the work of Dilthey and Weber. And Dilthey's conception of historicity is more "historical" than Heidegger's existentialist version, of which Olafson makes such good use.

Olafson's book represents a major advance in analytical philosophy of history. His treatment of the relationship between history and the social sciences, in the first section of chapter 4, is an example of the illuminating analysis that he provides throughout. Historians who put aside "the instinctive hostility of the practitioner to any characterization in broad conceptual terms of the enterprise in which he has proprietary rights" will find the book of considerable interest.

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GUY HOWARD DODGE. *Benjamin Constant's Philosophy of Liberalism: A Study in Politics and Religion*. Chapel Hill: University of North Carolina Press. 1980. Pp. xii, 194. \$16.50.

This study of Benjamin Constant is an admirable introduction to a neglected but interesting system of

early nineteenth-century political liberalism. Guy Howard Dodge considers Constant's system coherent in all its essentials, arguing that the vacillations in the writer's private life were irrelevant to the constancy of his liberal theory. Central to this theory, Dodge proposes, was the assertion of limited popular sovereignty, in deliberate contrast to Jean-Jacques Rousseau's concept of a sovereignty both unlimited and inalienable (and hence, Constant pointed out, largely inoperable). Constant decried the collectivist trends and implications of Rousseau's thought and insisted on retaining a firm line between popular sovereignty and individual rights and liberties. The dangers of Rousseau's system were all too well corroborated, said Constant, by the dictatorship of Napoleon Bonaparte. Moreover, sovereignty expressed in popular majorities would be scarcely safer than that claimed by despots, for nothing could legitimize the invasion of liberty, even by a majority.

The evidences of empiricism that Dodge finds in Constant's theory of sovereignty were stronger in his constitutionalism. Here, after 1814, his theory closely reflected the realities of Restoration France, dominated by a propertied, educated middle class in a conservative, constitutional monarchy. Constant advanced the concept of five governmental powers—executive-ministerial, judicial, legislative (split to represent both permanence and public opinion), and royal. The royal power should be neutral yet active, intervening primarily in political crises.

Dodge's view of Constant's liberalism focuses finally on the religious ingredient. At first sharing the antireligious bias of the French Enlightenment, Constant became more sympathetic toward religion after the turn of the century and indeed came to consider it an essential foundation for his liberalism. Intelligent self-interest (whose predominance in the Enlightenment may be overestimated by Dodge) might well support despotism rather than liberty and must be countered by the sacrificial moral imperatives of Christianity. Further, Constant was horrified by an atheism that could only lead, he maintained, to political cynicism and Machiavellianism.

Dodge is a skillful, lucid writer. The book, to be sure, is rather slight; Paul Bastid's *Benjamin Constant et sa doctrine* (1966), with all its biographical and literary concerns, nevertheless includes a great deal more on political thought than does the Dodge volume. Nor are many of Dodge's conclusions new to readers of Bastid or even of such a brief study as William W. Holdheim's *Benjamin Constant* (1961). What Dodge does add to earlier works on Constant are accessibility, serious documentation, firm placement of his subject in the context of three centuries

of liberal theory, and a lengthy but unannotated bibliography. The book apparently went to press before David K. Lowe's important *Benjamin Constant: An Annotated Bibliography* (1979) was available.

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IAN CUMMINS. *Marx, Engels, and National Movements*. New York: St. Martin's Press. 1980. Pp. 205. \$27.50.

Marx and Engels are not very useful to non-Marxist theorists of nationalism. They never devoted more than peripheral attention to the subject, and it may even be doubted that they fully appreciated its character. Engels was surprised that the Polish uprising of 1863 produced such strong anti-Polish feelings among the Russian peasantry, when class loyalty should have evoked support for the Polish attack on autocracy. Later in the same decade he called nationalism simply a "Bonapartist invention." Despite this, Marx and Engels did evaluate national movements consistently. They supported movements that strengthened the likelihood of the great European revolution they anticipated, and they opposed movements that seemed to impede the revolution. This is the basic thrust of Ian Cummins's brief book.

Marx and Engels favored national consolidation when it could lead to the strengthening of the class necessary to the eventual emancipation of civil society (Germany); when it would rid the English working class of an unfortunate distraction (Ireland); or when it might undermine conservative regimes such as the Ottoman Empire (Serbia) or tsarist Russia (the populists). But they were not sympathetic to opponents of progressive events such as the Hungarian revolution of 1848 or the penetration of capitalism into backward areas. It might be "sickening" to see the apparently inoffensive Indian social structure destroyed, but not when one recalled that the Indian village "restrained the human mind within the smallest possible compass" (p. 73). Engels characterized the Habsburg Slavs as "useless" and "historyless" peoples, because in 1848 these "ruined fragments" collaborated with the monarchy in crushing the Hungarian revolution and because he feared the lack of vitality of these "relics" left them prey to the panslavist schemes of Russia.

Cummins's discussion of these and other points is straightforward but colorless. He does not invest his material with anything more than its modest intrinsic interest. This will be a useful book for students who want a convenient summary of what Marx and Engels wrote on national movements,

but scholars interested in either Marxism or theories of nationalism will not find much meat.

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GEORGE RUDE. *Ideology and Popular Protest*. New York: Pantheon. 1980. Pp. 176. Cloth \$10.95, paper \$4.95.

George Rudé's long commitment to the history of popular protest has produced important and stimulating studies and inspired many others. His analyses of the identity of the crowds involved in eighteenth- and nineteenth-century riot and rebellion in France and England reveal that such action often was an effort to protect old rights or claims to justice. Protesters also made claims to new rights, but more rarely. In this study he moves beyond *who* rebelled, and *why*, and seeks the "underlying body of ideas . . . the 'ideology of protest.'" The study is divided into four parts. The first briefly examines Marxist theory, as developed by selected scholars, and concludes that Gramsci's concept of "non-organic" ideology (unstructured, relatively unfocused, sometimes confused) offers a useful key to understanding the rebellious action of the popular classes, in a polity in which the "fundamental classes"—bourgeoisie and proletariat—are not completely polarized and conscious. Rudé follows Gramsci also in his historical analysis by insisting that each situation be examined afresh and, finally, in his belief that ideology is superstructure.

Rudé identifies two strands of ideology: *inherent*, or popular, and *derived*, or structured. He believes both that some kind of popular ideology is always present and that there is no evolutionary progression from simple to more sophisticated ideas. The three remaining sections examine protest and rebellion of peasants in Europe and Latin America, revolution in England, France, and North America, and strikes and protest in England during the transition to industrial capitalism.

Although he has established a framework for examining ideology, Rudé has no method for independently identifying it. Consequently his analysis is circular, for he derives ideology from the affiliation of participants in protest, their economic interests, and the form of their action. His conclusions are familiar. They are those of his earlier work: backward-looking efforts to protect rights are the most common form of pre-industrial protest; in Europe, the most typical form changed gradually from tax to food riot with contemporary efforts of state or upper class to extract resources; revolution consists in the confluence of popular action and leadership from upper-class, conscious intellectuals with other grievances in the overthrow of a regime. Although

Rudé expresses little sympathy for what he calls "a certain fashion . . . to apply ready-made models to revolutionary situations and to leave the specifics to look after themselves" (p. 83), his own common-sense mistrust of both idealism and nominalism leads him to models of his own. Hence although his typology is richly descriptive, the study adds little to the debate on popular protest.

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PIERRE SORLIN. *The Film in History: Restaging the Past*. Totowa, N.J.: Barnes and Noble. 1980. Pp. xiii, 226. \$21.50.

Film is an illusion of reality. It is a carefully crafted lie. It does not reproduce reality so much as it interprets it. Films communicate the beliefs, values, and basic assumptions about reality—present, past, and future—of the society and time period in which they were created. Most traditional historians argue that fictional feature films are useless as historical documents because they misrepresent the historical events. At best they are tolerated as popular images of the past, equivalent to popular novels that use history for a background. This book by French historian Pierre Sorlin challenges historians to take fictional feature films seriously as evidence of historical understanding and illustrations of how societies felt about history and specific historical events. For Sorlin, films—like any other product of the mind—contain a philosophy of history. "By looking at historical films (fictional films about real historical events and people) we are able to describe the basic historical knowledge of a society," he argues. Every historical film is an indicator of "the country's basic historical culture, its historical capital."

Sorlin's interest is not in comparing the historical event in the films with an accurate description of the event. He is interested in comparing the film with the version of history given at the time the film was made—how a country through its films depicts its most important historical events. Sorlin looks at several events—the French Revolution, the American Civil War, the Italian Risorgimento, the Russian Revolution, and the Italian resistance movement—and how French, American, Italian, and Russian films depicted these events. The image of history presented in films is shown to reveal clearly how the past was popularly conceived in the period in which the films were made. The films discussed are *Birth of a Nation*, *La Marseillaise*, *1860*, *Grand Illusion*, and *The End of St. Petersburg*.

Sorlin's book is a brave attempt that is filled with problems. It is naive about many things, but Sorlin is courageous enough at least to allow the question

of the relationship of fictional film and the history profession to be taken seriously. He utilizes the latest methodological approaches to social ideas with understanding, and he has looked at the films carefully to attempt to coax from this mystifying medium relevant meanings. The works of Sorlin, of several people in England (such as Tony Aldgate), and of those historians associated with IAMHIST (International Association for Audio-Visual Media in Historical Research and Education) stand in sharp contrast to those of American historians. Unfortunately, the words Robert Rosen and I wrote back in 1973 are still applicable today: "If several million consumers are willing to pay hard cash to purchase a given ideational product, it would be extremely arrogant for historians to assume that the product contained nothing of interest." Sadly, the arrogance remains.

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GEORGE M. KREN and LEON RAPPOPORT. *The Holocaust and the Crisis of Human Behavior*. New York: Holmes and Meier. 1980. Pp. 176. \$18.50.

So intractable are the problems posed by the Holocaust that thus far the probings of social scientists have yielded few answers. Not unexpectedly a skillful use of an interdisciplinary approach for an event of such complexity presents the reader with a more perceptive vision. This book by George M. Kren and Leon Rappoport, the former a professor of history and the latter of psychology, uses that approach to maximum advantage, and the result is a welcome addition to the growing class of catastrophe history.

A series of problems that have emerged from the study of the Holocaust in the last decade are held up for scrutiny from the historical and psychological perspective. Examined are such problems as the search for historical causation, which encompasses several subproblems including the problematic link between German antisemitism, culture, and history and what happened at Auschwitz. Like others, the authors encounter difficulty in establishing a direct link. The role of the pathological personality of Hitler, without whose presence Auschwitz could not have happened, poses an even greater problem. What role did his ability to impose that murderous hatred on the German people through a totalitarian conduit play? Were they made ready by their history to receive and act on such a message? There is an insightful discussion of behavior in *extremis* and survival in the world of the concentration camp. The question of resistance and collaboration (which is producing a minor literature in its own right) is probed in an innovative discussion.

Given such a broad range of topics, one can note an unevenness in quality. The handling of some topics is almost inspirational, others more mundane. The quality of the narrative is characterized by clear, uncluttered prose and good transitions from topic to topic. A book of this kind has little place for original research, but it should make use of the pertinent secondary sources. There are some glaring omissions in this area.

In an incisive concluding chapter the Holocaust is viewed as a historical novum that breaks through conventional notions about the nature of man and society. Its specific historical valence relates to Western civilization whose industrial processes and ethos allowed it to dominate the world. What we witness in Auschwitz, after all, is that same industrial process gone awry. It processes to death the subgroup that produced the modernizing elite behind that system in disproportionate numbers. It is an ultimate truth about the meaning of the Holocaust that Kren and Rappaport examine with a refreshing degree of detachment.

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JOHN A. GARRATY. *Unemployment in History: Economic Thought and Public Policy*. New York: Harper and Row. 1978. Pp. xii, 273. \$15.00.

John A. Garraty begins this study of "how the condition of being without work has been perceived and dealt with" (p. xi) since ancient times with a discussion of the problem of defining unemployment, an elusive phenomenon that encompasses underemployment (short time, seasonal work, and underutilization of skills) as well as joblessness. In the pre-industrial era the concept of unemployment was merged with that of poverty with confusion of cause and effect. Nor was poverty the plight only of the unemployed. A medieval definition of the poor was "those who have only their work" (p. 23), and modern observers, beginning with Thomas Malthus (1798), described the working poor. A distinction was drawn—sometimes officially, as in Great Britain in 1359, 1834, and 1905—between the deserving and the undeserving poor. The latter were able-bodied shirkers. Some capable persons, to be sure, shunned work but most joblessness among employables was involuntary. The prevailing view, however, from the Athenians to a Massachusetts labor statistician (1879) and beyond was that unemployment was the idle individual's preference. Some writers—Eugène Buret (1840), Louis René Villermé (1840), Denis Poulot (1872), and William H. Beveridge (1909)—were ambivalent on responsibility for unemployment, but the general perception of the

able-bodied unemployed as loafers meant insistence that they be put to work. Various work-relief measures were tried. They were harsh and usually failures in every important respect.

Why was unemployment the idle person's own fault? Because there was work to be had by anyone who was willing to labor—a medieval assumption that died hard over centuries. Classical economists and empirical social researchers shared this assumption and did not consider unemployment a major problem. Marx, on the other hand, concluded that technological unemployment produced a reserve army of excess labor and that unemployment was necessary and normal in a capitalist system. Since his solution, the replacement of the system, was unacceptable to most economists, his analysis discouraged attempts to understand how unemployment might be reduced or eliminated. The focus in the early twentieth century was on "reducing the pain" (p. 139) through such devices as unemployment insurance, which was adopted in Great Britain in 1905 and Germany in 1927.

Meanwhile, investigation of the business cycle convinced Arthur Cecil Pigou and Wesley Clair Mitchell that moderating the cycle was the way to combat unemployment. In the 1920s economists noted that when prices rose, unemployment fell, and "the tension between this observation and the fear of inflation governed thinking about unemployment" (p. 155), preventing an assault on the problem through increased public spending. Then the depression set the stage for John Maynard Keynes, and from 1945 to about 1970 the Keynesian trade-off between unemployment and inflation worked. Keynes's achievement was estimable but time-bound, and eventually inflation ended the Keynesian golden age. In a chapter on unemployment today, Garraty asserts that until economists come up with new ideas that will deliver us from the worst of both worlds—high unemployment and high inflation—we should be less fearful of unemployment than of inflation.

This thoughtful and lucid study, drawing on a vast literature in several languages and referring to twenty nations, meets a real need. Garraty merits our commendation and his book a wide readership.

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PIOTR S. WANDYCZ. *The United States and Poland*. (American Foreign Policy Library.) Cambridge: Harvard University Press. 1980. Pp. ix, 465. \$25.00.

This volume is a part of the "American Foreign Policy Library" series and is organized accordingly. It provides the American reader with a comprehensive review of Poland's modern history, while at the



same time focusing particularly on those episodes in American history that have been directly linked with that of Poland. The story of the Polish immigration to America is also treated marginally—being discussed only insofar as it has played a significant role in relations between the two countries.

Within those limits, however, the volume offers an exhaustive survey of American-Polish relations. After briefly sketching the sporadic contacts between the two nations in the eighteenth and nineteenth centuries, it highlights three important episodes in more recent history: Wilson's contribution to the re-emergence of the Polish state during World War I, Roosevelt's acquiescence to the subordination of Poland to the USSR at the end of World War II, and, finally, the role played by Poland in American-Soviet relations in the postwar period. Each of those episodes is richly documented and presented in a clear and remarkably balanced fashion.

In a final part—which he deliberately calls “reflections” rather than “conclusions”—Piotr S. Wandycz provides an overall synthesis of Polish-American relations. He observes that there has been an essential asymmetry in the history of the two nations. Poland declined in power, and even temporarily completely lost its independence, at the very time when the United States came into existence. While American influence has grown steadily, Poland has never regained its position as a great power—in fact, it has been repeatedly threatened by its stronger neighbors, Germany and Russia. As a result, the Americans' policy toward Poland has largely become a function of their relations with the Germans and the Russians.

Despite many differences in their historical experiences as well as the great distance separating the two nations, the “Polish question” has often occupied an important place in American global foreign policy. This is partially owing to the extensive human contacts established by a sizable Polish immigration to the United States. To some extent, it is also due to the American ideology supporting the rights of smaller nations to independence. Most importantly, however, it stems from Poland's very strategic location—between Russia and Germany—in Europe. Wandycz rightly concludes that there can be no viable European order without a secure Poland as a part of it. The recent crisis in East-West relations over Poland has once again confirmed this view.

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RICHARD WICH. *Sino-Soviet Crisis Politics: A Study of Political Change and Communication*. (Harvard East Asian Monographs, number 96.) Harvard University:

Council on East Asian Studies; distributed by Harvard University Press, Cambridge. 1980. Pp. viii, 313. \$15.00.

It is one of the ironies of history that the People's Republic of China and the Soviet Union vie for leadership among the anti-imperialist states and yet are themselves leading imperial powers. Their empire building has led to their having the longest common border in the world. That, in itself, is an important reason for conflict, and the resulting Russo-Chinese antagonism has been a constant feature of history for centuries, except for very brief periods such as that following the successful communist revolution in China in 1949.

Although Richard Wich is undoubtedly aware of the historical background of the conflict, he is more interested in the adversary relationship (following the brief Sino-Soviet alliance of the 1950s) that developed in the mid- to late 1960s and how it affected the international scene. The first substantive chapter deals with the Soviet invasion of Czechoslovakia in 1968 and the last concerns the 1970–72 period. Wich stresses the border crisis of 1969, which, he says, was a political concoction of the Chinese. As they emerged from their self-imposed isolation during the Cultural Revolution—the war in Vietnam was winding down, the U.S. was pulling out, and the Soviets were increasing their influence—Chinese leaders used the Soviet invasion of Czechoslovakia and its justification, the Brezhnev doctrine, to alter bipolar political relations in their favor.

The events and polemics that the author details, however, are a vehicle for his larger purpose, which is to stress the importance of the “signaling process” for a better understanding of political change. Scholars and diplomats should give more attention to what world leaders say, not just to what they do, because such “early warning signals” precede actions and can, therefore, be used as a guide for developing policy. This is a risky business indeed, given the subtlety and uncertain outcome of such signaling. But Wich argues his point well and thoroughly, if not always in straightforward, understandable prose. This, as well as his dismissal of the broader historical context, will trouble some historians.

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LAWRENCE S. KAPLAN. *A Community of Interests: NATO and the Military Assistance Program, 1948–1951*. Washington: Office of the Secretary of Defense. 1980. Pp. xii, 251.



This study, part of a series prepared by the Office of the Secretary of Defense, traces the origins of NATO, with background on world political developments, 1945–47, military and economic assistance programs, and the influence of the Truman Doctrine. It then describes the complex process leading to the creation of NATO, the impact of the Korean War, and the intense debates domestically and abroad over the structure of the alliance and how it should develop.

The work is indicative of a new type of combined military and diplomatic history. It masterfully reveals the complex and interwoven processes and planning that involve multilateral treaties of alliance, and it reveals how closely the Departments of State and Defense and other agencies and governments had to work both to achieve accords and implement them. Lawrence S. Kaplan's account focuses on these interrelationships, offering much more data on internal U.S. processes than on the positions, perceived threats, and maneuvers of other signatories. He relies upon previously unavailable memos and reports, including National Security Council materials, minutes of meetings and correspondence, plus memoirs and congressional hearings. The Department of Defense had to assess needs, weigh the strengths and weaknesses of allies, administer military assistance programs, and formulate plans for an integrated European army. The State Department concentrated upon negotiations and is portrayed by Kaplan both as more sensitive about problems and more arrogant regarding its role.

The most troubling feature lies in the title, which stresses "a community of interest," while the narrative emphasizes frictions, conflicting views, contradictory concerns, and obstinacy. While Kaplan provides excellent descriptions of the policy-making processes that reconciled the differences, the administrative strains revealed in NATO's early years have continued to appear in the gaps between commitments and realities. Kaplan often criticizes the petty nature of some of the rivalries that contributed to delays, but he rarely passes judgment on who was right or wrong.

Scholars interested in administrative detail will glory in the precise description of operations; general readers will discover information on Greece and Middle East policies, Europe's programs toward integration and American efforts to advance that goal, the complex problem of rearming Europe and rebuilding it economically, Truman's leadership, and the structure and problems of NATO. Kaplan also provides incisive assessments of other scholarly interpretations of the NATO alliance. The study is also a useful reminder of a significant transition period when national leaders had to abandon old practices and policies and accept the new doc-

trine of interdependence. There is a valuable bibliographical essay and appendixes of the Brussels and NATO accords, the Mutual Defense Assistance Act of 1949, and other measures related to defense assistance, including the National Security Act of 1951.

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## ANCIENT

THEODORE A. WERTIME and JAMES D. MUHLY, editors.  
*The Coming of the Age of Iron*. New Haven: Yale University Press. 1980. Pp. xix, 555. \$22.50.

This concise collection of detailed studies edited by Theodore A. Wertime and James D. Muhly is dedicated to Cyril Stanley Smith, a noted pioneer in the investigation of materials science. The fourteen invited papers attempt to cover the introduction of iron and its technology in many cultures and over a vast span of time. The papers divide into two broad categories, those that deal with analytical data derived from the examination of recovered artifacts and those that examine linguistic and literary references to iron and its compounds. Both are instructive to historians and technologists alike. The opening essay, "The Pyrotechnologic Background" by Wertime, is an overview of the enormous amount of research into the ancient technology relating to the ubiquity and character of iron ores and the making of iron and steel. Although the host of references is impressive, it is hoped that the author will extend the scope to the dialectical distributions found in the Sumero-Akkadian lexical lists edited by B. Landsberger, M. Civil, and their successors.

The following essays, "The Bronze Age Setting" by Muhly and "The First Archaeological Appearance of Iron and the Transition to the Iron Age" by J. C. Waldbaum, take the two different avenues of approach. The first reviews many ancient literary sources and discusses the terms for "iron" appearing in a number of ancient texts. Unfortunately, the length of the paper does not allow for a fully satisfying treatment of the many philological problems and the troublesome phenomenon of the transfer of loan words of technological significance from one cultural-linguistic group to another. The second paper allies with the next one, "Metallurgy and Ancient Man" by T. S. Wheeler and R. Maddin, in utilizing the results of contemporary metallurgical laboratory methods to examine ancient artifacts. Unfortunately the carefully made scanning electron photomicrographs (SEM) do not reproduce at their best on the paper stock of the book. One interesting

conclusion that the authors draw states, "On the basis of our research and that of others, it is apparent that smiths were carburizing intentionally on a fairly large scale by at least 1000 B.C. in the Eastern Mediterranean area" (p. 121). The following paper, "Ocher in Prehistory: 300,000 Years of the Use of Iron Ores as Pigments" by D. Schmandt-Besserat, seems out of place, for it attempts to survey too much material from too many disparate sites produced over too great a length of time. Unlike the other chapters in the book, the citations vary in quality of sources; some are older primary papers, others popular works on culture and antiquity. The methodology of the study is clearly out of phase with the other studies in the book. The following two essays, "The Coming of Copper and Copper-Base Alloys and Iron" by J. A. Charles and "Furnaces, Crucibles, and Slags" by R. F. Tylecote, are straightforward and worthwhile reading. The review of the work pioneered by B. Rothenberger at Timna in the Negev is especially interesting. Possibly the most innovative article in the book follows, "An Alternative Sequence for the Development of Metallurgy: Tepe Yahya, Iran" by D. Heskel and C. C. Lamberg-Karlovsky, which reviews the findings at one of the few excavations where a long usage of metals through a number of cultural occupations can be surveyed. After presenting both cultural finds and metallurgical analysis, the authors state, "At Yahya throughout the 4th millennium, where almost all known copper-bronze metallurgical techniques are utilized (except sulfide ore extraction), there are no major changes observable in the archaeological record which reveal quantitative or qualitative changes in the social institutions" (pp. 262-63).

The subsequent papers review the evidence for the status of iron technology in a number of representative areas of historical significance. Among these are "The Central Andes" by H. Lechtman, "The Mediterranean" by A. M. Snodgrass, "Europe" by R. Pleiner, "Western Iran" by S. Pigott, "Africa" by N. J. van der Merwe, and "East and Southeast Asia" by J. Needham. The evidence for early African metallurgical innovation has been noted in the scientific press in the past few years, and van der Merwe offers a good summary of the research. The last paper, however, seems in many ways the weakest in the book, although the author's credentials are, of course, impeccable. It is totally dependent upon Chinese literary sources and simply passes over the exciting recent developments in Chinese archaeology. It is after all, however, the one less successful contribution in an otherwise very interesting and informative book.

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PAUL FRIEDRICH. *The Meaning of Aphrodite*. Chicago: University of Chicago Press. 1978. Pp. xi, 243. \$13.95.

Paul Friedrich believes that scholars have tended to avoid the serious study of Aphrodite because of "deep cultural and religious biases," which have led them to reduce this powerful goddess to "a fun girl or a patroness of prostitutes." Following hints from two exceptional Greek scholars (Snell, Farnell) who were not so inhibited, Friedrich salutes Aphrodite as "more important in popular cult than any of the other goddesses" and proposes to apply the methods of the "cultural anthropologist, linguist, humanist, poet, and specialist in Homeric Greek" to an exploration of her origins.

Readers of Friedrich's exemplary monograph on the Proto-Indo-European arboreal system (1970) will not be surprised to find that the sections of the present book based on the analysis of linguistic evidence (chap. 2, on Sanskrit *Ushas*, Greek *Eos*, Latin *Aurora*; appendixes 2, 5, and 6, on *charis*, *eos*, and *philommeides*) carry the greatest conviction. Much more attention is devoted, however, to cultural anthropology and structuralism than to comparative linguistics, and it is likely that the persuasiveness of the book as a whole will vary according to the interest and experience of the individual reader (perhaps also his tolerance for a style rich in such expressions as "syncretic-cybernetic," "Aphroditoids," and "intersubjectivity" and much concerned with such topics as mother goddesses who patronize procreation, and the Old European Water Bird as a forerunner of Aphrodite).

It would probably be fair to quote a sentence (p. 103) as indicating the author's general approach: "The fact that in many cases it is impossible to demonstrate conclusively the precise nature of the connections between specific antecedents and the form they later took in the early Greek syntheses does not deprive relatively informed hypotheses of their interest." In this liberal spirit the author proceeds from the figurines of Old Europe (7000-3500 B.C.), previously analyzed by Gimbutas, through a survey of the standard archaeological sources for Sumerian, Semitic, Minoan, and Mycenaean analogues, to an explication of the principal early Greek texts (Homer, Hesiod, the Homeric Hymns, Sappho) and a structural analysis of four goddesses (Hera, Athena, and Artemis, as well as Aphrodite), to whom are applied eighteen "semantic dimensions" in a process designed to answer the question, "What did Aphrodite mean to an appreciative and informed Greek in Homer's audience?" (p. 100). Thereafter the perspectives of contemporary psychology are directed to a study of the liminality of Aphrodite and the contrast between this goddess

and Demeter, in an attempt to trace the two to a common source.

A series of nine appendixes, ten pages of notes, twelve of bibliography, and an index that includes important Greek terms conclude the book, whose value for a classicist lies partly in its broad historical context and its suggestive, admittedly hypothetical interpretation of comparative material, partly in its reading of Sappho, to whom, however, the academic world is not nearly as negative as the author suggests on page 126. (The persistent confusion of Leto with Leda [pp. 81, 154] is presumably a misprint.)

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GEORGE CAWKWELL. *Philip of Macedon*. London: Faber and Faber. 1978. Pp. 215. \$19.95.

George Cawkwell's many learned articles published in the twenty years preceding the publication of *Philip of Macedon* have established his reputation as an expert on fourth-century Athenian domestic politics and foreign affairs. In this interesting and stimulating book, Cawkwell has provided the classical scholar and the general reading public with a fascinating and provocative new biography of Philip II of Macedon, whose reign (359–336 B.C.) marks the emergence of Macedonia as a major power in the Greek world. In fewer than 200 pages of actual text, Cawkwell traces step by step the rise of Philip and the transformation of Macedon from a weak, disunited kingdom of negligible political and military importance into the most formidable power in the Western world.

The book is a first-class scholarly monograph, thoroughly researched, fully documented, enriched with sensible insights and carefully and concisely argued. Its author is well acquainted with the geography and topography of Macedonia, the ancient sources, and the voluminous modern literature on the subject. To keep the length and cost of the book down, however, the author has severely rationed the scholarly apparatus of the book. Thus, the notes at the end of the book give the ancient evidence fully but rigorously eschew all modern discussions except those that provide necessary archaeological information.

Although Cawkwell's views throughout the book are generally sound, the reader familiar with the subject will find a few statements of questionable validity. This reviewer, for example, cannot accept the author's view (p. 169 and following) that the League of Corinth was based exclusively on a *koine eirene* (common peace) treaty. In my book *The Unification of the Greeks under Macedonian Hegemony* (p. 94 and following), I have presented sufficient evidence

to show that the League of Corinth was based both on a *koine eirene* treaty and a *symmachia* (offensive and defensive alliance). Likewise, one might quarrel with the author's contention that the Chalcidians could not have been saved even if Athens had heeded Demosthenes' advice and helped them in the early days of their war with Philip "with all her might to the best of her ability" (p. 88). Since Demosthenes' advice was not followed—or rather it was badly followed—we cannot be sure that the Chalcidians could not have been saved even with strong Athenian help. Finally, I disagree with Cawkwell's conclusion (p. 163 and following) that as a master of tactics Philip was as resourceful as his son Alexander, while as a master of strategy he far surpassed Alexander. I do not think that the information we have from ancient sources is sufficient to enable us to compare the two men as tacticians or strategists.

Such minor strictures aside, this is an excellent book and an important addition to the corpus of Philip studies available in English.

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SUSAN M. SHERWIN-WHITE. *Ancient Cos: An Historical Study from the Dorian Settlement to the Imperial Period*. (Hypomnemata, number 51.) Göttingen: Vandenhoeck und Ruprecht. 1978. Pp. 582. DM 95.

Historians are aware that Cos was the reputed home of Hippocrates, but little else about this important center of Greek intellectual and religious life has been available outside specialist literature. Thus one greets this thick tome with gratitude. *Ancient Cos*, however, could be half its 582 pages and yet convey the essential material; the lengthy summaries of scholarly controversies could easily have been confined to footnotes with the problems of primary sources discussed in the text.

The book has major strengths and makes important contributions toward comprehension of Coan political history, particularly concerning the vexing questions of the relationships between Hellenistic Cos and Ptolemaic Egypt. One may contest Susan M. Sherwin-White's conclusions about the favor shown by Ptolemy II toward Cos, based upon Theocritus and other literary sources, but her arguments are precise and well buttressed by judicious employment of disparate bits of epigraphical evidence. Specialists in Hellenistic state relations may well regard chapters 2, part 1, and chapters 3, parts 1–3, as definitive.

By contrast, the contents of chapter 7, "The Coan School of Medicine," sure to draw the attention of scholars curious about the background of Hippocrates and the so-called Hippocratic school of medi-

cine, is an unfortunate mishmash of undigested material that indicates faulty understanding of the medical sources and an overreliance upon the recently disproven fancies of Herzog. Readers new to the controversies surrounding all aspects of Hippocratic medicine will be puzzled by Sherwin-White's indecisiveness on whether or not there was medical instruction at Cos, whether or not the Hippocratic physicians were associated with the priestly colleges of the Asclepian temple, and just what it was that the *iatroi* in the inscriptions did to gain such adulation. One is indeed grateful for the assembly of epigraphical evidence on public physicians, but this long chapter adds very little to the conclusions of Pohl, Cohn-Haft, and Nutton. It appears that Sherwin-White has been seduced by a common presentism that states that doctors must, somehow, "make sense" in a modern setting or they are not physicians at all. Chapter 8, "Coan Religion," is a good summary of the numerous cults, but it leans heavily on the still standard account by Farnell, occasionally on Nilsson, and, for the all-important Asclepius cult, on Edelstein (even though Sherwin-White professes disagreement).

One may read with profit chapter 5, "The Coan Constitution," but certain sections of chapter 6, "Economic Aspects," betray a lack of mastery of technically problematic sources (similar to chap. 7). Wine, silk, and perfumes receive attention; but without control of botanical, pharmaceutical, agricultural, and entomological specifics, the survey here does no better than Forbes's often questionable summaries. Sherwin-White senses deficiencies, as she adds the appendix 2, "The Silk Trade of Ancient Cos," which further compounds the confusion, especially because reliance upon Forbes's wobbly botany and entomology is simply increased. One must understand Dioscorides in tandem with Aristotle, Galen in conjunction with Ovid and Scribonius Largus, and Theophrastus' *Historia Plantarum* to comprehend what Greeks and Romans did with perfumes and oils, what they thought of worms (including the silkworm), and why wines had specific qualities noted as late as the Byzantine *Geoponica*. This reliance upon secondary sources is in striking contrast to the sure foundations on epigraphy and the Hellenistic literature that characterizes chapters 2, 3, and 5.

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PETER R. POUNCEY. *The Necessities of War: A Study of Thucydides' Pessimism*. New York: Columbia University Press. 1980. Pp. xv, 195. \$19.50.

Peter R. Pouncey has combined modern scholarship on Thucydides and his own analysis to construct a sensitive study of the historian. Thucydides characterized war as "a harsh teacher, [which] adapts the passions of most people to their actual situation" (3.82). Pouncey explores how Thucydides traces this adaptation in cities and individuals during a generation of war. The reader is prepared by several unusually sound chapters giving essential background: the effect of the war on Thucydides' own life, his methodology, and the tension between achievement and decline (for example, between Pericles' Funeral Speech and the following chapters on the plague). The methodology chapter outlines Thucydides' techniques for universalizing particular events, with notes on recent scholarship. The central chapters study major figures and events that lay bare the underlying fear and self-interest Thucydides sees in human nature. The "cyclical process of rises and falls, prompted by need, greed, or the will for power" (p. 48), found in the *Archaeology*, is pursued in the preliminaries to the war, the greatness of Pericles, the aggression of the Melian dialogue, Alcibiades' self-interest, and the fears of Nicias and Phrynichus. The richness and variety of these chapters derives from Pouncey's skill at interrelating, as Thucydides does, each topic with numerous others in other parts of the history, linking, for example, Pericles to Cleon and Nicias, or the Melian dialogue to the statements in the Mytilenean speech at Olympia. The concluding chapter, "Human Nature at War," argues that the collective fear, aggression, and self-interest of cities is a reflection of the same qualities in individuals. An appendix treats Thomas Hobbes's similar view of human nature.

This book, because of its brevity, general scope, elegance of style, and focus on a central issue, deserves to become a standard introduction to Thucydides. Nevertheless, many aspects of Thucydides' work are omitted: not only all historical questions but also major themes such as naval power, the relation between planning and chance, and the difficulty men experience in knowing the truth. Pouncey perhaps trusts the speeches too much as the chief source for Thucydides' thinking, not always using them in conjunction with the narrative. Thus he misinterprets Nicias's words at 7.77.2-4 (pp. 128-29, compare 94-96) that it would not be proportional to the human things (*anthropeia*) the Athenians had done for the gods to punish them further. Pouncey takes this as confirming that even the pious Nicias agreed with the generals at Melos, that the gods worked according to human laws (compare Th. 5.105.1-2). But Nicias's words must be weighed against the facts. The Athenians do suffer more, much more. Thucydides, like the tragedi-



ans, knew that suffering is not in proportion to human action but much worse. Nicias's words, tested by the facts, reveal how men delude themselves with hope in times of stress.

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HERMANN BENGTON. *Die Flavii: Vespasian, Titus, Domitian. Geschichte eines römischen Kaiserhauses*. Munich: C. H. Beck. 1979. Pp. 316. DM 38.

The Flavian dynasty, as Hermann Bengtson points out in his introduction, constitutes an important epoch in Roman history. It was these three rulers who put the empire back together again after the convulsions of A.D. 69, and so laid the foundations on which the five "good emperors" built, the men normally credited with responsibility for Rome's golden age. But as he also remarks, there has been no comprehensive study of the dynasty for years, only the occasional monograph on one or another of the three rulers, while it is A.D. 69, the Year of the Four Emperors, that has attracted most attention recently. At the same time, however, Bengtson is fully aware that imperial biography is under attack now, on the ground that it enforces too narrow a focus for an adequate account of Roman imperial history. Hence, the fifteen chapters of this book intermingle the actions and policies of the Flavians with the principal economic, social, intellectual, and religious developments of the period. Eight pages of brief notes give the main sources, ancient and modern, on which Bengtson has relied; there is also a chronological table, a family tree, two maps, and ten well-chosen plates.

As a whole, the book is best described as a work of "haute vulgarisation," a serious, comprehensive study of the period that avoids the minutiae and the quagmires of scholarly controversy. Specialists will obviously find fault with some of Bengtson's views. He seems, for example, far too favorably disposed toward Titus. Conceding that Titus was fiscally irresponsible, he excuses his handling of his brother Domitian and discounts Dio's comment (66.18) that Titus's reputation depended largely on the very brevity of his reign. Bengtson also shows impatience with prosopography on one occasion at least (p. 299, n. 17), and this may have led him to undervalue recent studies seeking to show that Domitian, not Titus, was the true heir to their father's policies. Again, when Bengtson discusses Flavian foreign policy, he shows no awareness of Edward Luttwak's groundbreaking study, *The Grand Strategy of the Roman Empire* (1976). Similarly, when he discusses the intellectual climate of the time, Bengtson

limits himself to descriptive comment, when others have discerned considerable significance in the bloody-mindedness of the court poet Statius. The economic situation too could probably have been made much clearer if Bengtson had devoted a separate chapter to the empire-wide census of A.D. 73, rather than scattering the material piecemeal through the book. Finally, there should have been more discussion of how Nerva was able without challenge to seize the throne after Domitian's assassination.

Despite such carping, it should be emphasized that this is a good book. Bengtson undoubtedly catches the essence of Vespasian and his reign; he makes a valiant, sometimes fascinating attempt to come to grips with Domitian's character and policies; and he makes good use of numismatic, epigraphical, and archaeological evidence to flesh out and to balance the picture provided by literary sources. Specialists, students, and general readers will benefit from reading this work.

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KARLHEINZ DIETZ. *Senatus contra principem: Untersuchungen zur senatorischen Opposition gegen Kaiser Maximinus Thrax*. (Vestigia, Beiträge zur Alten Geschichte, number 29.) Munich: C. H. Beck. 1980. Pp. xxv, 421. DM 86.

Maximinus Thrax was born a Thracian peasant and was advanced in the army by Septimius Severus because of his gigantic size and his physical strength. When a military revolt in Gaul overthrew Alexander Severus in March 235, the troops in Germany saluted Maximinus as emperor at Mainz. The new Emperor spent his reign fighting the Germans on the Danube with great success. His rule at Rome was characterized by substantial oppression, however. A counterrebellion broke out in Africa in 238, which led to the proclamation of Gordian I and his son as emperors. The Senate at once recognized the two Gordians because they feared the uneducated Maximinus. When Maximinus moved into Italy, he was stopped at Aquileia by strong local resistance. His troops soon grew tired of the civil war and put Maximinus Thrax and his son to death in April of 238.

The literary sources (Herodian and the *Historia Augusta*) have given us the answers to why the majority of the Senate took the occasion of the Gordians's usurpation to dethrone Maximinus Thrax. In this new work, however, Karlheinz Dietz makes use of prosopographic information such as the career, origin, and social standing of the members of



the Senate to determine why the Senate was opposed to Maximinus. Dietz has based his investigation upon the original prosopographic studies such as Pierre Lambrechts's dissertation *La Composition du Sénat Romain de Septime Sévère à Dioclétien* (193-284) (1937) and Guido Barbieri's *L' albo Senatorio da Settimio Severo a Carino* (193-285) (1952). Lambrechts's work is a most general survey of the third century determining only origin and year of consulship for the members of the Senate. But it must be kept in mind that his efforts were the pioneering acts in the area of prosopographic study. Barbieri's opus is a more detailed undertaking that includes origin, consulship, references in literary sources and inscriptions, and family background for the senators of the third century.

Dietz's work is a superbly detailed prosopographic study of eighty-seven members of the Senate for the reign of Maximinus Thrax. He has added a new African senator named Cosinius Marcianus (Dietz no. 29). Domitius Antigonus (Dietz no. 31) is Barbieri's number 33 and 1017, and Dietz gives us the full names for [T?] Cuspidius Flaminius Severus (Dietz no. 30) and [L?] Ragonius Venustus (Dietz no. 74). The vast majority of the Senate continued to be Italian in origin, elected to the consulship, and members of the official Roman priesthoods. The only question that this reviewer could raise was the placing of A. Caecina [Tacitus?] (Dietz no. 14) before Q. Caec[ilius] Pudens (Dietz no. 15) in the author's list.

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### MEDIEVAL

JOSHUA PRAWER. *Crusader Institutions*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xv, 519. \$89.00.

Those familiar with Joshua Prawer's work over the last three decades and with the focus of his *Latin Kingdom of Jerusalem: European Colonialism in the Middle Ages* (1972) will readily appreciate this new work. *Crusader Institutions* also treats the kingdom as a colonial product but does so more intensively, concentrating on five topical areas elaborated on in nineteen chapters. Although almost half of the chapters have been published previously, virtually all of them have been reworked in some way by rewriting, translation, or bibliographical updating. The remainder are new studies, most of which amplify the topical areas still further.

This ensemble's extended discussion of a somewhat narrow agenda, coupled with a style strongly

influenced by criticism of and commentary on sources, will principally interest scholars. What is most likely to excite the admiration of professional students of Outremer is the coverage Prawer gives to the status and administrative role of burgesses, including nationals of the Italian communes. At least twelve chapters, six of which have not been published before, deal in some way with this subject, which standard institutional histories of the Latin kingdom like those of Dodu, La Monte, Mayer, Richard, and Prawer himself reveal to be in need of further treatment. Of central importance is the chapter on burgrave tenure. There is also extremely valuable information on the development of the burgess class and the patriciate, on the administrative functions performed by such men, on their legal traditions, and on their urban environment. In no part of the book does Prawer apply his tools of textual criticism and painstaking use of charter evidence to greater advantage than in his handling of the subject of burgesses.

Elsewhere there are some aspects of the work that deserve challenge. The author's attribution of the Court of the Chain's inauguration to King Amaury I (1161-74) as probable rejects without explanation charter evidence associating the *catena* with the early twelfth century, evidence that he himself apparently once accepted (*Histoire du Royaume Latin de Jérusalem* [1969-70], vol. 1, p. 500 and note; R. B. Patterson, *Speculum*, 39 [1964]: 474-77). Many medievalists no longer consider a refutation of John L. La Monte's image of the Latin kingdom of Jerusalem as an ideal feudal state to be necessary. Not a few of them will also reject the line of argument used. The irony about the reservation one feels about the assault on La Monte is that it is partly due to Prawer's own scholarly contributions that he can in this case be held gently to account.

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DAVID BERGER, editor and translator. *The Jewish-Christian Debate in the High Middle Ages: A Critical Edition of the Nizzahon Vetus with an Introduction, Translation, and Commentary*. (Judaica, Texts and Translations, number 4.) Philadelphia: Jewish Publication Society of America. 1979. Pp. xviii, 422, 164. \$20.00.

This is the fourth volume in the series "Texts and Translations" published by the Jewish Publication Society of America. Like Gerson Cohen's classic, *The Book of Tradition by Abraham ibn Daud* (1969, third in the series), David Berger's edition of the *Nizzahon Vetus* (to be translated either as "old book of polemic" or "of victory") gives us a critical Hebrew text and an English translation, a very substantial

commentary, an introduction, indexes, and five appendixes. The treatise under discussion, a particularly aggressive anti-Christian polemic, was composed sometime between 1275 and 1325. The anonymous author referred to previous Jewish polemics from France, yet he himself must have lived in Germany, as suggested by many German words and phrases inserted into the text. In 1681 J. Wagenseil published the polemic in his *Tela ignea Satanae*, based on a manuscript from Strasbourg. Until recently scholarship was in want of a new edition. Now we have two of them, for in 1978, while David Berger's book was presumably in press, Morderhai Breuer of Bar Ilan University, Israel, came out with his critical version of the *Nizzahon Vetus*.

The treatise is arranged in two distinct parts. The first covers most books of the Bible, starting with Genesis and ending with Psalms and Song of Songs, looking for passages that might be subject to christological interpretation. Once the Christian point of view is presented, the Jewish reader is armed with a response. The second part of the book presents a very critical reading of the Gospels: the anonymous author quotes some of the passages in Latin or German (in Hebrew letters), offers the necessary translation, and points out the weaknesses of the text. These polemical discourses are in general quite short, of only 150 to 250 words. While Berger edited 153 paragraphs in the first part and a total of 245, Breuer (who altered the order of the Wagenseil text and added to it materials from other manuscripts) reached a total of 280 items.

Social historians will be particularly interested in the last paragraphs of the *Nizzahon Vetus*, where textual critique is abandoned and attention is given to allegations against medieval Jews. We learn (for example, in paragraph 238) that it is accepted by some people, Jews and non-Jews alike, that "most gentiles [are] fair skinned and handsome while most Jews are dark and ugly." Christians (in paragraph 236) criticize Jews "for not confessing the way they do" and, what is more severe, they "anger us by charging that we murder their children and consume the blood." Jews, on their part, should not hesitate to refer to the corruption of the clergy (paragraph 238) and are invited to refute Christians "with regards to their practice of taking the bones of the dead as holy relics" (paragraph 240). In paragraph 245 an answer is provided to those who "criticize us by saying that the Talmud distorts and spoils our entire Torah and prevents us from realizing the truth by leading us astray." Most scholars see in this a reference to the trial of the Talmud in Paris in 1240.

Berger's commentary depicts the intellectual history of each argument and indicates its parallels in the vast corpus of Jewish-Christian polemics of the Middle Ages. His knowledge of Hebrew and Latin

sources, together with his familiarity with the results of modern scholarship, account for his remarkable achievement. It is Berger's merit, however, to have raised the possibility that, besides its polemical message, the book also might have had the aim of "fortifying the faith" of hesitant Jews. That Berger refrains from addressing himself to this issue might have to do with the poor state of our knowledge of the social history of conversions to Christianity during the Middle Ages. Further study of the question, its dimensions, and its patterns, may help us seize in full one day the background against which the *Nizzahon Vetus* was compiled.

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RICHARD W. UNGER. *The Ship in the Medieval Economy, 600-1600*. London: Croom Helm or McGill-Queen's University Press, Montreal. 1980. Pp. 304. \$32.95.

Another worthwhile contribution has been made to the rapidly expanding field of medieval technology. In six chronologically arranged chapters, Richard W. Unger has traced maritime developments over the millennium from 600 to 1600. Carefully researched and well illustrated (including a suspiciously modern appearing drawing of a Byzantine warship from ca. 850) and with an extensive bibliography, the book provides a good synthesis of current information on nautical technology, based on underwater archaeology and kindred sciences, as well as a ransacking of the specialized literature and journals in French, German, Spanish, Dutch, the Scandinavian languages, and English.

Years of research and travel and considerable support from foundations and individuals went into the writing of this book. Developments in hull construction are traced as carefully as the evidence allows, as are changes in sails and rigging. Although technical and foreign nautical terms are well explained, a glossary nevertheless would be useful to the nonspecialist reader. The organization of the book is unusual in that each chapter has two sets of footnotes: one to elucidate the text and another for the illustrations.

The author notes the decline in techniques that resulted from the abandonment of the Roman practice, in which a finely built, watertight shell formed the hull and a frame was inserted later. Shipbuilding continued in the early Middle Ages in the Mediterranean, but it sought easier and cheaper methods. This resulted, by about 600, in the gradual adoption of the frame-first, or skeleton technique, whose plank covering required caulking and frequent repair.

Unger pays considerable attention to the development of that remarkable technical achievement of the northern seas, the clinker-built Viking ship with overlapping, flexible planking. The unusually seaworthy Viking ships gave their possessors advantages in trading as well as in raiding, but their potential for further development was limited. Attempts to increase their size and carrying capacity were unsuccessful. The future lay with broader, higher, deeper ships with sternpost rudders and greater carrying capacity, such as the *cog*. Improvements in ships were paralleled by the introduction of better navigational instruments. By 1300 the magnetic compass and the *portolan* chart made voyages safer and more predictable.

Increasing dependence on sail propulsion rather than oars called for less manpower, brought multi-masted ships with increasing numbers of both fore-and-aft and square sails, and led to the "great invention" of medieval shipbuilders—the full-rigged ship (p. 216). Larger ships with many sails not only provided an efficient platform for cannon but were also more easy to control. By the late fifteenth century Europe was ready to undertake long-distance trading and exploring voyages with far-reaching effects for the future history of the world.

Unger has brought together much little-known information about the highly important and hitherto inadequately treated subject of medieval shipbuilding in this valuable monograph.

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PAUL R. HYAMS. *King, Lords, and Peasants in Medieval England: The Common Law of Villeinage in the Twelfth and Thirteenth Centuries*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1980. Pp. xxii, 295. \$49.50.

The broad title of this book is misleading. It requires the subtitle, "The Common Law of Villeinage in the Twelfth and Thirteenth Centuries," to indicate that it is essentially a specialized piece of legal history. Its substance is an exposition of plea roll cases relating to villeinage in the later twelfth and the thirteenth centuries, set against the rationalizations and theories of the lawbooks of that period. Paul R. Hyams here postulates villeinage as a legal term of art signifying the common law of serfdom. Serfdom is seen as the correct term to express only the social and economic aspects of the condition he is describing. Social, political, and economic factors are not ignored, for the author deliberately set out to rework and amplify Paul Vinogradoff on villeinage (to Vinogradoff, developments in law

were both an influence on social facts and a consequence of them). Hyams has certainly striven to relate his legal material to the facts of everyday life for medieval villeins, but the book reveals how rarely these conformed to common law theories or practices. His main achievement is that he elucidates Vinogradoff's suggested third and final stage of villeinage, which Vinogradoff rightly believed was both artificial and late, the classification of society under two heads: freeholders protected by the common law and villeins excluded from it.

This book demonstrates how the operations of the English common law developed a legal theory of servile villeinage between about 1160 and the end of the thirteenth century, when it appeared fully fledged in the Edwardian Year Books. The theory was an amalgam of seigniorial custom, royal interest, and legal expertise, but it was not a conscious creation of kings or of class oppression; it was created for administrative convenience by lawyers' ingenuity. Just as Robert Hoyt showed in 1950 that the exalted form of villeinage in ancient demesne was a product of Angevin times and did not exist before the later twelfth century, so now Hyams cogently argues that ordinary, servile villeinage was a common lawyers' body of doctrine that originated in the late twelfth century and was only fully developed in the thirteenth. He also uncovers the sources of the "Bractonian" theory of villeinage, incidentally confirming the views of S. E. Thorne that the *De legibus et consuetudinibus Angliae* was not the work of one man but a composite work compiled and revised over a number of decades.

The continuous handling of legal argument and case exposition on which the book is based requires the reader's closest attention at all times, and it is never easy going for the general historical reader, in spite of the generous lacings of chapter summaries and the recapitulations provided. Its conclusions, nevertheless, are important for English medieval history. Hyams's book does not have the facility and range of Vinogradoff's *Villainage in England*, but it is now the essential supplement to it.

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JANET MEISEL. *Barons of the Welsh Frontier: The Corbet, Pantulf, and Fitz Warrin Families, 1066-1272*. Lincoln: University of Nebraska Press. 1980. Pp. xix, 231. \$19.95.

Janet Meisel has written an account of three families whose major lands were in Shropshire, within the chronological limits of the Norman Conquest and the death of King Henry III. Following an account of the history and descent of the Corbets, Pantulfs, and Fitz Warins, the lands of each family

are discussed. Finally, a comparative analysis of the three case studies is offered.

The Corbet family was established in western Shropshire in the early 1070s by Corbet the Norman, who came to hold land from Roger of Montgomery, earl of Shrewsbury. Corbet's son, Roger, was the founder of the barony of Caus, the family seat. A most interesting member of the family was Thomas (d. 1274). His sister, Margaret, was married to Gwenwynwyn, prince of Powys, and another kinswoman was married to Llywelyn ab Iorwerth, prince of Wales. Thomas Corbet held seisin of his lands for over half a century without paying relief for them, made war with his neighbors, entered into a truce (like a prince) with the prince of Wales in the late 1220s, and was an important figure in the defense of England against the Welsh. Thomas extended both the geographical extent and the legal form of his territorial liberty in the March and usurped such full authority as to be virtually independent of royal power. Meisel estimates that in the thirteenth century the Corbets held a group of clustered lands in England, mostly exempt from royal authority, covering something like 150 square miles; Welsh lands, of uncertain extent, were held in addition.

The Pantulfs arrived in England in the Conqueror's reign, having apparently been vassals of the house of Montgomery in Normandy, and William Pantulf became the first baron of Wem by the early 1070s. Over the next four generations William's descendants cut increasingly modest figures in the March until William II (d. 1233) doomed his dynasty by failing to beget a son. Like those of the Corbets, the majority of the Pantulf lands were in Shropshire, but they were concentrated in the northeastern quarter of the county. Like the Corbets, the Pantulfs obtained substantial lands from Roger of Montgomery and profited from the spoils following the failure of Robert of Bellême's rebellion in 1102. At that point the two families were at near parity in landholdings, but the Pantulfs did not succeed in further expansion.

The Fitz Warins were not to be found in the Welsh March as early as the Corbets and Pantulfs. From the second quarter of the twelfth century, however, they steadily increased their holdings in land. It was Fulk Fitz Warin I (d. 1171) who about 1140 became lord of Whittington and Alberbury, the lands with which the family is usually identified. It was Fulk III (d. 1258) whose successful rebellion against King John in the first years of the thirteenth century (to regain control of Whittington) made him the central figure of the *Romance of Fulk Fitz Warin*. Fulk's second revolt against John did not end as well as the first, but he was able to re-establish himself in royal favor and to pass an augmented patrimony on to Fulk IV (d. 1264). The

Fitz Warin lands were more scattered and held of a greater number of lords than those of the Corbets and Pantulfs, a reflection of their later arrival in England. Yet the Fitz Warins were so successful in expanding their landholdings that by the mid-thirteenth century they apparently held more land than the Corbets and Pantulfs. Although the majority of the Fitz Warin land was not in the Welsh March, from the time that Whittington was acquired the March became the geographical center of family activity and concern.

It is Meisel's thesis that the connection with Wales was the primary element in determining the influence and power of these three families and that the Corbets and Fitz Warins played a greater part in thirteenth-century politics than has been recognized hitherto. Roger of Montgomery, who was given virtually all of Shropshire by the Conqueror, chose to place Roger Corbet as his vassal with responsibility for protecting the Vyrnwy and part of the Severn valleys from Welsh attack, and enfeoffed Corbet with lands along the border from Bausley on the north to Montgomery on the south. It was the potential of Welsh attack and the necessity of Corbet resistance that afforded the family the opportunity to increase its importance, while the relative security of the Pantulf lands away from the western frontier denied them the chance to turn a military value into political power. The Corbets were able to become lords of Welsh lands and of Welshmen and to serve as intermediaries between the English and the Welsh. Meisel theorizes, in fact, that the Corbets spent a substantial portion of their income on Welsh and English mercenaries in the thirteenth century, showing that they fully understood the military and political currents of the time. Fulk Fitz Warin III also understood the value of border lands and risked rebellion against the king for the chance to secure Whittington. Meisel argues that the Corbets and Fitz Warins grew in prominence in the 1220s and 1230s as the Welsh threat to English security became greater and that they remained important as long as the hostility between England and Wales continued. So important were conditions on the March, Meisel suggests, that, in addition to a regicentric analysis of the history of the wielding of authority in twelfth- and thirteenth-century England, the March of Wales and its lords must have their place, and Edward I could only weaken the power of the Marcher lords by conquering Wales and ending the Anglo-Welsh confrontations.

Meisel has made a significant contribution to the historiography of the March of Wales, and her conclusions deserve to be tested by further studies of other Marcher families.

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J. L. BOLTON. *The Medieval English Economy, 1150-1500*. (Everyman's University Library.) London: J. M. Dent and Sons or Rowman and Littlefield, Totowa, N.J. 1980. Pp. 400. Cloth \$25.00, paper \$16.00.

Now that it is clear that there will be no posthumous E. M. Carus-Wilson volume in the Longman series, this book by J. L. Bolton has every chance of surviving for many years as the best available single-volume textbook, offering a synoptic view of the later medieval economy.

B. A. Holderness's *Pre-Industrial England* appeared in the same format from the same publisher in 1976, although these volumes do not formally comprise a series and—invariably—match poorly at the seams around the dividing year, 1500. With E. Miller and J. Hatcher's *Medieval England* (1978) still incomplete, university teachers have had very little else to set before students except Michael Postan's *Medieval Economy and Society*, published in 1972 but in essence formed out of his celebrated and seminal Cambridge lecture course of the previous generation.

In basic ideas, especially in its chronology of expansion and contraction, this book does not set itself against the Postan norms. Bolton sees change injected into the economy mainly through growth and relaxation of demographic pressure on the domestic side and growing international and inter-regional division of labor resulting from commerce, with urbanization in its train. Bolton also follows Postan in emphasizing crucial regional and local differences in economic structure and in the tempo of change.

Bolton provides only 166 numbered references, all at the end of the book. Many are simply cross references or glosses; none are to archival sources. The work, nevertheless, is clearly based on a thorough knowledge of the published literature up to 1978, as the 32-page, chapter-based bibliography indicates.

It is modestly and soundly argued, with proper emphasis on areas where sources leave doubt or ignorance the only honest man's habit. There are no graphs, few tables, and no algebra; in other words it is a literate person's book. The style is not exciting but rather that of a sober lecture course with rhetorical questions, enclitics, and pointer conjunctions smoothing the way.

MAURICE BERESFORD  
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ANNE DUGGAN. *Thomas Becket: A Textual History of His Letters*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xxii, 318. \$49.50.

Anne Duggan's book is a painstaking, meticulously researched study, clear in its arguments and in its conclusions, and an indispensable tool for scholars at work on the Becket controversy or on any of its principals. The author collates the important manuscript sources and explains their relationships both among themselves and with the contemporaneous biographies so heavily dependent upon them. The first part of the book deals (too sketchily) with Becket's correspondence in the light of other twelfth-century epistolography and then moves on to discuss the various collections of his letters, both printed and manuscript. Part 2 demonstrates the deep obligation of Becket's early biographers—Grim, Herbert of Bosham, William of Canterbury, William FitzStephen, Guernes de Pont-Sainte-Maxence, Roger of Crowland—to the letter collections. Valuable appendixes and tables (including what must be the standard tabulations of the chronology of the correspondence) and excellent indexes conclude this learned work.

Yet, the book ultimately is troubling; for whom was it intended? It is so narrowly focused and so technical that it would be of real interest only to Becket scholars not to historians of the twelfth century or even to those of Henry II's reign. We are told far more than we need to know: colors employed in the manuscripts, erasures, scribal indentifications (unlike T. A. M. Bishop's *Scriptores Regis*, this work contains no plates), and other tiresome minutiae march drearily by. Surely this study's value, and its place in the literature, will be enhanced when a new edition of the Becket correspondence is published. Until that gladsome event, Becket scholars will find this work chiefly valuable for the chronological tables.

JAMES ALEXANDER  
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EVELYN S. PROCTER. *Curia and Cortes in León and Castile, 1072-1295*. (Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1980. Pp. xvi, 318. \$42.50.

Well regarded for the essays on Castilian and Catalán constitutional history she produced over a long and active career, Evelyn S. Procter made it the goal of her retirement years to accomplish a comprehensive analysis of the Castilian Cortes. Publication of the resulting volume is posthumous, her death having taken place in March 1980 during the final stage of proofreading.

Procter has left us a closely reasoned account solidly based on contemporary archival sources. Taking her cue from the stages through which the Cortes are seen to have progressed, she adopts a chronological, geographical, and topical organiza-



tion for her findings, devoting chapters to the *curia regis* in León-Castile from 1072 to 1157; the *curia* in the separated kingdoms of León and Castile from 1157 to the reunion of 1230; Leonese and Castilian towns in the thirteenth century; attendance of town representatives at the great assemblies of León and of Castile before 1250 and at the Castilian Cortes from 1250 to 1295; the composition, procedure, and business of the Cortes during the same period; and the *curia* (vernacular *corte*) from 1230 to 1295. A concluding chapter places Castilian constitutional development in the context of the Spanish peninsula, drawing comparisons and contrasts with representative assemblies in the lands of Aragon, Navarre, and Portugal. The comparative scope of the work is enlarged by frequent allusions in preceding sections to analogous constitutional trends in England and on the Continent.

The picture emerging from these considerations is that of a royal *curia* that persisted throughout the period under study as the "main governmental institution" (p. 7) even as it adapted to the growing role of Cortes. Procter rightly notes as "by far the most important development in the history of the *curia*" (p. 46) the attendance of townsmen, first attested to at Leonese assemblies in 1188, 1202, and 1208 and at Castilian meetings in 1214, 1217, and 1219. A reluctance to assert more than extant texts prove leads Procter to take a more cautious position than Joseph O'Callaghan ("The Beginnings of the Cortes of León-Castile," *AHR*, 74 [1969]: 1503-37) regarding the initial appearance of townsmen at the Cortes in León-Castile, the earliest use of proctorial powers by town representatives, and the continuity of their being summoned to the Cortes before 1250.

Thirty-two assemblies held between 1250 and 1295, a period of more rapid development and frequent meetings compared to earlier periods, serve as the basis for an extended analysis of the composition, procedures, and functions of the Cortes with particular attention being paid to the role of town representatives. Although Procter affirms that the need for revenue may have been the "strongest single inducement" (p. 266) for kings to summon the Cortes, she argues convincingly for "administrative convenience" (p. 267) as the strongest impetus for the thirteenth-century development of representative assemblies in León-Castile and in the entire Spanish peninsula. The appendix presents nine documents, all but two previously unpublished, including the fifth and last of Alfonso X's economic codes.

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F. JAVIER FERNÁNDEZ CONDE. *Gutierre de Toledo, Obispo de Oviedo (1377-1389): Reforma eclesiástica en la Asturias*

*bajomedieval*. (Publicaciones del Departamento de Historia Medieval, number 3.) Oviedo: Universidad de Oviedo. 1978. Pp. 484.

F. Javier Fernández Conde is well known for earlier works on the ecclesiastical history of Asturias. His book is based on extensive use of Vatican and Spanish ecclesiastical and secular archives.

Gutierre Gómez de Toledo (ca. 1330-89), a member of an important noble family of Toledo, was related to other leading prelates of the day. He was typical of his age in that both his appointment as bishop and his relative success as a reformer were largely due to royal support. In return, Don Gutierre proved invaluable to Juan I of Castile in the king's struggles against rebellious relatives and foreign enemies in the 1380s. Although Fernández Conde documents this "political" side of Don Gutierre (and the bishop's success in consolidating his episcopal seignior) more fully than has been done before, this part of the book contains little that is substantially new.

The longest and most interesting chapter (pp. 137-234) deals with Don Gutierre as an episcopal reformer. Fernández Conde publishes the bishop's *Libro de las constituciones* (Oviedo, Archivo Capitular, MS. 6) for the first time (pp. 311-450). The *Libro* contains the acts of five diocesan synods (1377-84), statutes for the cathedral chapter of Oviedo, and a series of constitutions reforming the main male and female religious houses of the diocese. These documents are accompanied by an apparatus that lists variants found in other partial contemporary copies in the same archive. Other manuscripts are used to present complementary reforming measures and the bishop's catechism, drawn up for the use of his clergy. Almost all these documents are in Castilian rather than Latin. None of this is particularly original. Fernández Conde adduces much comparative Castilian material—he does not use recent work on the Crown of Aragon—to show how the bishop's activities should be seen within the context of contemporary church reform. But it is unusual and valuable to have an episcopate so well documented.

The documents used cannot tell one everything. Without contemporary diocesan registers recording the day-to-day activities of the episcopal curia—and these registers appear not to exist for Oviedo in this period—it is difficult to say how effective the bishop's legislation was. He certainly did his best. Unlike many of his contemporaries and despite his political responsibilities, he visited his diocese himself. It does seem that it was hard to make either the secular clergy or religious houses take even the most elementary prescriptions—in the case of the clergy canons against absenteeism, concubinage, and indifference to excommunication—seriously. The

bishop was obliged to suppress two Benedictine female convents entirely.

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LÉON FLEURIOT. *Les origines de la Bretagne: L'émigration.* (Bibliothèque Historique.) Paris: Payot. 1980. Pp. 353.

Breton is the only Celtic language still spoken on the continent of Europe. Why? To a large degree this excellent book was written to answer that question. Actually, this is a history of early Brittany that will replace the century-old work of Joseph Loth and supersede the last book of Nora Chadwick, a book that she did not live to finish and polish.

The problem of the settlement of Brittany from England has been misunderstood, Léon Fleuriot argues. Brittany was not a *cul-de-sac* but a *carrefour* of maritime routes encompassing both sides of the English Channel. Brittany had always had a substantial Celtic population that was only partially supplanted by the Romans, and settlement of the peninsula from Britain was a long and regular process. The émigrés thus did not come as strangers, and they found in Brittany people who spoke similar languages and who had similar cultures and institutions. As a result, a fusion was possible that resulted in a stable and relatively homogeneous community that has persisted to our own times.

Fleuriot lays great emphasis on the soldiers who came to Brittany in the trail of Roman pretenders from 383 to 411. In addition to the many insular folk who had settled widely on the continent, and not just in Armorica, these soldiers formed a first wave of immigrants. Then in the sixth century came a second wave, connected with the Breton saints. In the past scholars have concentrated their attention on this second wave. This book shows that the chronology of the Breton migration must be extended considerably before and after the *termini* suggested by earlier scholars.

A second major contribution of the book is its demonstration that the first wave of Breton immigrants enjoyed good relations with the Romans, and then with the sub-Romans. These earlier Breton settlers joined with the Franks to combat the Visigoths and enjoyed long and friendly relations with King Chilbert. During this period both the Bretons and the Franks exercised influence in Britain and worked together to restrain the Saxons on both sides of the Channel. All this time the Bretons shared in the political, cultural, and ecclesiastical life of Gaul. They felt themselves to be Romans, Fleuriot believes.

After the middle of the sixth century, the Saxons became more menacing and relations between the Bretons and Franks deteriorated. Thus it is from the

mid-sixth century that one must date the emergence of a more distinctively Celtic society in Brittany with its, in Frankish eyes, peculiar religious and cultural practices. Things might have turned out very differently had Franco-Breton relations remained good.

In addition to these grander lines of interpretation, the book makes other large and small contributions. For those who do not like to talk of invasions and migrations but prefer to speak of the peaceful accommodation of foreign peoples into the late Roman world, this book provides support. Those interested in the persistence of ethnic communities within the Frankish world will find much food for thought here. Scholars concerned with the uses and applications of personal names and place-names will discover in Fleuriot's early chapters a masterful discussion of the value of these kinds of evidence. Fleuriot manages to show how much is fact and how much fable in the story of Conan Meriadec. The murky figure of Riothamus is masterfully treated; Fleuriot believes that he was none other than Ambrosius Aurelianus. Breton evidence is adduced to support an early rather than a late date for the baptism of Clovis. One could go on and on. This is a rich and insightful book.

Inevitably there are some flaws. French does not make the distinctions English does between Briton and Breton, and Britain and Brittany. Once in a while Fleuriot's meaning is obscure because of an uncautious use of these words. In several places Fleuriot promises a second book on the internal history of Brittany during the period (roughly) 400–750. It is a real shame that more internal history and especially religious history are not included in this book. La Borderie's treatment of these subjects was fanciful in the extreme. Fleuriot is a sane and judicious scholar, and it would have been good to have had his treatment of early Breton history integrated into his discussion of *l'émigration bretonne*.

The book concludes with three valuable appendices. The first is a catalogue of the sources for early Breton history that contains occasional extracts from the documents and, more importantly, information on editions, texts, and scholarship on the sources. Second, one finds a long and detailed chronological table that displays in a clear fashion the arguments developed in the text. Third, a series of maps illustrates the major lines of interpretation developed in the book. There is also a very good bibliography.

THOMAS F. X. NOBLE

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HERMANN FRÖHLICH. *Studien zur langobardischen Thronfolge: Von den Anfängen bis zur Eroberung des italienischen Reiches durch Karl den Grossen (774).* In two volumes.

Tübingen: Eberhard-Karls-Universität. 1980. Pp. 289; 265.

Hermann Fröhlich's study of the successions of Lombard monarchs is a very valuable piece of basic research. It has been carefully and thoroughly researched, and no detail has been spared in the effort to compile a full dossier on every change in the Lombard monarchy. The background material is also presented in detail. Fröhlich has an admirable knowledge of the sources of Lombard history, and he has demonstrated considerable skill in the critical analysis of the sources. Although not everyone will agree in complete detail with the conclusions that Fröhlich has reached, all who study his work will agree that his views must be treated with respect. Where he errs, it is clearly through caution rather than carelessness.

The greatest strengths of Fröhlich's work are the exhaustive discussions of the literature. Each episode in the history of the Lombard monarchy is discussed in the light of each of the traditional sources, which are then compared and minutely criticized for accuracy, the contexts in which they were composed, and the biases of the authors. In addition, all of the major items of the historical literature on these questions are similarly discussed in a most thorough manner. Perhaps the major shortcoming of this work is that, in some instances, Fröhlich attributes a too precise use of terminology to some of the sources. This is not by any means a serious problem. In short, this is a very good dissertation, worthy of consultation by Lombard specialists and those interested in early Germanic rulership. I recommend it highly.

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CHRISTINE E. MEEK. *The Commune of Lucca under Pisan Rule, 1342-1369*. (Speculum Anniversary Monographs, number 6.) Cambridge: Mediaeval Academy of America. 1980. Pp. vii, 127. Cloth \$11.00, paper \$5.00.

Separated into eight parts, this study of some 110 pages of text and 28 pages of notes is a nuanced expansion of pages 3-7 of the author's *Lucca, 1369-1400* (1978). The introduction contains a useful survey of recent historical treatments of the rule of Italian subject cities (especially those controlled by Milan, Florence, and Venice). It also includes descriptions of the principal archival guides and of major source collections in the Archivio di Stato of Lucca that should serve a wide variety of scholars.

Christine E. Meek intends "to show what Pisan rule meant for both the Pisans and the Lucchese" and the "benefits and burdens" that accrued to each city. This she attempts in six chapters, "The Organization of Pisan Rule in Lucca," "The

Administration of Lucca under Pisan Rule," "The Economy," "Finance," "External Relations," "Lucca under Giovanni dell'Agnello (1364-69)," and a conclusion. The book unfortunately has no index.

Meek's conclusions contain few surprises. In general, "Pisa put the security of its position in Lucca first, but apart from that was prepared to allow Lucca autonomy in local and internal affairs. . . . [Lucca's] freedom in economic affairs contrasts favourably with the position of Florentine . . . or . . . Venetian [subject cities]" (pp. 122, 123). Lucca was "generously treated" in finance. Pisans did not establish themselves socially or economically in Lucca. Pisa made few institutional changes and did not try to "establish joint institutions."

What the author demonstrates is documented thoroughly. A major problem, however, derives from what may have been haste to conclude and to make this a very short book. Important facets of the problem could have been studied in depth. Interesting findings are not seized upon or examined for results that would prove significant for more than an explanation of individual events in Lucchese history.

The author assumes that Pisa could have controlled Lucca more completely and directly had it wished to (pp. 53, 76, 78). There is little analysis of Pisan internal history and of the relation of that history to Pisa's Luccan policy. (Comparative information concerning the relative sizes, populations, and wealth of Pisa and Lucca, 1342-69, would have assisted the reader.)

We read that "Lucca was allowed to prohibit export of foodstuffs to Pisa even in times of shortage" (p. 60). But why? Why, too, was there "no attempt to subordinate the Lucchese guilds to their counterparts in Pisa, as happened in Pisa itself under Florentine rule" (p. 61)? After the peace of August 29, 1364, made with Florence "on terms unfavourable to Pisa" (p. 101), "Lucchese revenues were administered directly by Pisa for the rest of the period of Pisan rule. . . . Lucca apparently preferred this system, which denied it control of its own revenues" (p. 84). Why should this have been so? Perhaps more detailed sociopolitical and economic analysis of Lucchese history during these years will reveal the answers. And no one is better equipped than Meek to discover and elucidate them.

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WILLIAM ANDERSON. *Dante the Maker*. Boston: Routledge and Kegan Paul. 1980. Pp. xii, 497. \$45.00.

The title of this book is quite promising. It recalls Guido Guinizelli's words in the twenty-sixth canto

of the *Purgatorio* on Arnaut Daniel, "fu miglior fabbro del parlar materno." It likewise distantly echoes Aquinas's definition of art in the *Summa Theologica* (1-2, q. 57, a. 3), "ratio recta aliquorum operum faciendorum."

The structure of the work is appropriately Dantesque, being divided into three sections. After an introductory chapter in which Dante is proclaimed, in the words of Ruskin, "the central man of all the world," part 1 takes Dante through the composition of the *Vita Nuova*. Part 2 covers the poet's middle years and all of his works except the *Commedia*, to which the entire third part is devoted. There are some fourteen sketch-maps, graphs, and designs as well as 32 pages of notes, a bibliography, and an index. It has all the appearances, then, of a major scholarly contribution to Dante studies.

Unfortunately the book is seriously flawed. It is neither a straightforward introduction to Dante for beginners nor a probing study of his work for specialists. It falls somewhere in between and on either side. William Anderson clearly loves Dante, but he allows a fascination with either trivial or questionable material to lead him far afield. I fear that students may be seriously misled if they rely on this book alone and Dante scholars infuriated or amazed.

The author does not provide the information necessary for placing Dante in the full intellectual tradition of his time; for example, his relationship with the *ars dictaminis* and its poetic curriculum is not developed. Surprisingly, the book is poorly written, with a profusion of dangling participles, misplaced modifiers, and jarring similes. These deficiencies are capped, for me, by the author's indulgence in an attempted analysis of Dante's creative impulse. All kinds of literary (especially Russian) material are invoked as well as, to use the publisher's words, "recent discoveries in neuro-psychology." Far from being left, at the end of the book, like Neptune looking up at the Argo, this reader found himself lost in a "selva oscura," bristling with strange shapes.

For all of its promise, then, this book is disappointing. Much of what it includes should have been omitted; much of what it omits should have been included.

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ERIC CHRISTIANSEN. *The Northern Crusades: The Baltic and the Catholic Frontier, 1100-1525*. (New Studies in Medieval History.) Minneapolis: University of Minnesota Press. 1980. Pp. xv, 273. Cloth \$25.00, paper \$10.95.

More than one scholar has asked, half-puzzled, why there was no satisfactory modern narrative account

of the crusades in the Baltic region. The answer was partly self-explanatory: no press would publish a book for such a limited audience. The rest of the answer followed: no scholar would waste a career researching an unpublishable and difficult subject. Well, at last we have an author, a text, and a press. The University of Minnesota Press is to be congratulated for presenting this long-needed account of the Scandinavian, German, and Slavic crusading ventures.

The one complaint that can be raised is about the cover illustration. What would be perfectly appropriate on a dust jacket is printed black on blue, giving the impression that this is a children's book. Indeed, a child might read it, so well do the sentences flow, but it is a scholar's work—and a fine one.

The title tells it all: the story begins well before the first crusade (1147) and is extended to encompass Prussia, Livonia, and Finland; discussion of the religious attitudes of the era is mixed with an analysis of the frontier concept. How Archibald Lewis did not get into the footnotes or bibliography is hard to understand, because Eric Christiansen's topic and approach are logical continuations of Lewis's early works.

Christiansen has a chatty style, both informal and informing, that makes climbing a mountain of facts seem but a pleasant evening stroll. He exhibits a humane and even humorous spirit that avoids the strident righteousness that makes up so much of the literature. Probably a few readers will dislike this aspect of the book, since Christiansen refuses to judge and condemn or to mix modern politics with medieval history.

The author handles the extensive original sources with skill and familiarity—an accomplishment probably fully appreciated only by the handful of Baltic specialists. His touch is sure and confident, and, although he occasionally brings forth a generalization that is perhaps too daring, he provokes thought rather than anger; his command of the subject is such that he rarely goes wrong. Christiansen, with supreme authority, rules over a subject that the best of modern scholars seek to approach only through relatively narrow studies.

Nowhere does his command of the subject show better than in the description of the Swedish crusading endeavors. This survey has practically no predecessor in the English language. Quite properly, the author gives proportionately more of the text to this crusade than to any other.

Christiansen favors the Western student probably too much, even omitting German, Slavic, and Scandinavian sources from the reading list of secondary literature. It is obvious from his comments that he has read extensively in the literature, but the few footnotes are almost invariably citations from original sources.



The maps are excellent, the illustrations well chosen. For a one-volume survey of the Northern crusades, the reader has but one choice: this is it, and it is fortunately a good one.

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## MODERN EUROPE

MICHAEL A. MULLETT. *Radical Religious Movements in Early Modern Europe*. (Early Modern Europe Today.) Boston: George Allen and Unwin. 1980. Pp. xxiii, 193, \$19.50.

This book by Michael A. Mullett considers in four chapters how religious radicalism responded to the rise of the national state, socioeconomic changes, the establishment of Protestantism, and the question of toleration. The discussion ranges from the Waldensians to Robert Walpole, as early modern Europe is stretched elastically, but the author pays greatest attention to the English Dissenters, particularly the Quakers. He offers valuable discussion of the ways in which the radicals found themselves at odds with the major trends of their time. How could they welcome the increased power of the prince if that power was used against them? Since many were pacifists, how could they support the nationalist spirit that was the source of hatred and bitter wars? The new monarchies were the last things the dissenters desired, and so the elaborate apparatus of hierarchy, university, court, and pagantry were all anathema to them. The second chapter examines the radicals and socioeconomic changes and the debates on this since the Weber and Tawney theses were first proposed. The author stresses that the religious radicals in their fundamentalism were not at all laxist in their economic ideas but were strongly anti-acquisitive. Thus only some of them acquired wealth, most did not, and those who were the innovators in the economic sphere usually found themselves outside the pale of their religious community.

Mullett sees a tie between late medieval lay religious movements and the radical reformers. Both were usually at odds with the establishments: Catholic, Anglican, Lutheran, or Calvinist. The radicals' isolation, biblical literalism, as well as symbolic interpretation and spiritualizing of the Bible all had social and political implications for them. Finally, Mullett sees toleration in the early modern period as a sign of political weakness (as opposed to the modern omniscient state). He illustrates his discussion especially with reference to the Quakers and appends documents and commentary.

Some questions do arise out of the author's basic assumptions. Did religious radicalism lead to modern civilization in reducing the ceremonial aspects of life, or has ceremony merely taken on new forms? Can one be so optimistic in comparing the modern and seventeenth-century states and worlds? Is our world truly less violent? Did the Reformation tame the Hobbesian European, or was the Hobbesian man a product of that era? Still, this is a stimulating book and is useful both for the classroom and for personal reading.

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THOMAS MICHAEL LOOME. *Liberal Catholicism, Reform Catholicism, Modernism: A Contribution to a New Orientation in Modernist Research*. (Tübinger Theologische Studien, number 14.) Mainz: Matthias-Grünwald-Verlag. 1979. Pp. vii, 452. DM 86.

This is an indispensable work for all scholars interested in modern Roman Catholicism and particularly Catholic Modernism. The product of many years of indefatigable research, much of it in unpublished manuscript material, the book consists of three distinct yet related parts. Part 1 is an extended reflection on Modernism as a historiographical problem, and Thomas Michael Looome offers numerous judgments and theses on aspects of Modernist scholarship. Experts will find this section both stimulating and irritating. Looome views earlier historical work on the Modernist period as polarized between Romanist critics and Modernist sympathizers and calls for an "impartial" transcending of these "two Catholicisms." The severity, however, with which he treats the work of other scholars and the role of some Modernists—for example, the biblical scholar Alfred Loisy—belies Looome's appeal for disinterestedness. Furthermore, the notion of "two Catholicisms" is not a helpful historiographical model; it distorts rather than illuminates the complexities involved. Looome's knowledge of the German and English sources is prodigious, yet in such a rich field there are bound to be lacunae; some, however, are striking, such as his curious failure to consider important French sources and the influence of the work of certain German Protestant scholars and theologians on key Catholic Modernists.

The central thesis of the book maintains that Modernism was an aberrant episode in a larger movement, both historically and geographically, that can best be called the "liberal Catholic tradition." Looome stresses the fact that "Modernists" such as Tyrrell never thought of themselves as Modernists but rather as standing in the long tradition of liberal Catholicism. This reviewer, however,



would agree with other historians that the term Modernism should be retained as a *terminus technicus* in Roman Catholic historiography, since there were, in fact, certain shared notions about knowledge, history, metaphysics, and dogma that reflected the intellectual milieu of the period 1880–1910, such as its agnosticism, historicism, idealism, evolutionary developmentalism, and pragmatism, which were not representative of the early or mid-nineteenth century. Furthermore, the Roman church did condemn and seek to suppress certain philosophical-theological doctrines that had particular currency at the time and that, although perhaps in no case held as a coherent system by any Modernist, were, contrary to Looime, advocated by such influential teachers in the church as Loisy, Tyrrell, LeRoy, and Blondel.

Part 2 of the book comprises over one hundred pages of invaluable, specialized bibliographies on Modernism and liberal Catholicism—some material previously unknown and some conveniently brought together for the first time. Part 3 consists of two types of unpublished documentation: a catalog of unpublished sources for eighty-three persons and five institutions associated with Modernism and a selection of unpublished texts, including important statements from Lord Acton, von Hügel, Bishop, and Tyrrell. The first part of the book will serve as a provocative departure for thinking about Modernism; parts 2 and 3 will serve as an essential bibliographical guide for years to come.

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A. W. DEPORTE. *Europe Between the Superpowers: The Enduring Balance*. (A Council on Foreign Relations Book.) New Haven: Yale University Press. 1979. Pp. xv, 256. \$18.50.

A. W. DePorte's cautiously optimistic book is reminiscent of *Candide*: all is for the best in this best of bipolar European systems. Dominated by the United States and the Soviet Union, the bipolar system that emerged from the division of Europe after 1945 and from the Cold War has maintained peace in Europe and the independence of the European states for three decades. The consequent stability is preferable to any feasible alternative.

The traditional European balance-of-power system, in which no one state could gain ascendancy, lasted four centuries until 1914. The dramatic rise of German military power around the turn of the century upset this historic balance. The United States was needed in World War I to overcome German power, as were the combined efforts of the United States and the Soviet Union in World War II to help end the hegemony of Germany over the Continent and to destroy it as a power and a state.

The old European system was dead, but the domination of the two superpowers over divided Europe and the institutionalization of their presence in Europe led to a bipolar state system and stability. The individual states, voluntarily in one camp and under coercion in the other, have been dependent on one of the superpowers for political, economic, and security reasons. The problem of German unification has been resolved by the cementing of the two German states into the two blocs and by West German rearmament. The Euroatlantic subsystem, embracing military and economic arrangements, has become structured, stable, and useful to its members.

In spite of detente, the European Community, the policy of de Gaulle, extra-European conflicts, and changes in doctrine and in weaponry in the last twenty years, the bipolar balance in Europe has remained. The interests of most of the states have been served by this system, which accurately reflects existing power relationships.

*Europe between the Superpowers* is itself a model of balance and studied moderation. DePorte is no Cold-War ideologue or advocate of the menace of Russian communism. But he is equally critical of the simplistic revisionist view that most problems between the United States and the Soviet Union have been due to American deviousness or economic interest. This is not a book of original scholarship nor of startling insights, but it does deal judiciously with familiar problems. It is too short, however, to deal with some complex aspects of DePorte's theme. His analysis of de Gaulle as a proponent of European consciousness raising will puzzle the spirit of Jean Monnet. He will not pay the political price that Gerald Ford did in 1976 by describing the East European states as independent, however circumscribed in practice.

In DePorte's broad sweep of political history and, to a lesser degree, economic analysis, there is no place for the recent upsurge of nationalism or regionalism and little for the increasing and divisive impact of OPEC. But the generally sound judgment of the author and the logical skill and clarity with which he argues his case make his book a valuable contribution to contemporary history.

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C. R. ELINGTON, general editor. *The Victoria History of the Counties of England. York, East Riding*, vol. 4, edited by K. J. ALLISON, pp. xvi, 186. \$110.00; *Middlesex*, vol. 6, edited by T. F. T. BAKER, pp. xx, 228. \$125.00; *Shropshire*, vol. 3, edited by G. C. BAUGH, pp. xvi, 399. \$132.00; *Oxford*, vol. 4, edited by ALAN CROSSLEY, pp. xx, 506. \$132.00; *Somerset*, vol. 4, edited by R. W. DUNNING, pp. xx, 266. \$99.00; *Stafford*, vol. 6, edited by

M. W. GREENSLADE and D. A. JOHNSON, pp. xix, 294. \$129.00; *Chester*, vol. 2, edited by B. E. HARRIS, pp. xvi, 266. \$115.00; *Cambridge and the Isle of Ely*, vol. 7, *Roman Cambridgeshire*, edited by J. J. WILKES and C. R. ELINGTON, pp. xvi, 98. \$49.95. New York: Oxford University Press, for the Institute of Historical Research, University of London. 1978–80.

It has now been over eighty years since the first volume of *The Victoria History of the Counties of England* appeared; the volumes listed above bring the total number issued to 161. Thirty-seven counties out of thirty-nine (as well as London) are now represented by at least one volume. In sheer numbers it is possibly the most impressive enterprise undertaken by English historical scholarship.

The *History* has a checkered past that in itself provides an interesting footnote to the story of English historical studies in this century. (That story is well told in the *General Introduction* written by the present general editor in 1970.) Launched at the very close of the Victorian epoch, the *History* was given the queen's blessing, the right to use her name, and a royal subscription (the first on the list) for the whole series. It was intended to be sold to a small audience of subscribers, but that audience was not expected to be an academic one. The language of the *Description and Plan* published in 1907 is indicative of the editors' purposes. The full series, when completed (160 volumes as then conceived), would form "the first logically detailed presentment of that full sequence of events of which Queen Victoria herself indicated the magnificent development." The empire over which she ruled had grown out of "a Kingdom based upon her English Counties, in every one of which its characteristic growth, its typical developments may be observed and studied." The *Description* went on to add that there was "no acre of England that was not pregnant long ago with Empires yet to be."

If the work was to embody these patriotic and nationalistic sentiments, however, it was also to be a product of unimpeachable scholarly repute; listed among its advisory council were such names as those of Pollock, Firth, Round, and Poole. Among the early contributors such scholars as Haverfield, Round, and Leach appear conspicuously. Initial output was impressive: 38 volumes by 1907. But the founders' hopes of a self-financing enterprise and of relatively speedy completion began to fade. Increasingly it was the single-handed efforts of William Page, as general editor, that kept the project going. He piloted it through the First World War and the difficult decades that followed. For a time he edited the *History* without salary. Eventually, near the end of a long career, Page was able to interest the Institute of Historical Research at the University of London, and in the early 1930s it was officially

taken over by that body. But it was not until after the Second World War that the happy solution of obtaining public support from local government authorities was arrived at. First with the Wiltshire County Council (1946–47) and later with other county or borough authorities a partnership in finance and in editorial direction was achieved.

The results of Page's long and devoted direction were mixed. The output was large, reaching 100 volumes in 1938, four years after his death. But the geographical distribution told a less happy story. A comprehensive plan for thirty-nine counties (excluding Monmouthshire) had been laid out at the beginning. By 1936 eleven counties had completed this schema. In nine others an initial two volumes had issued, and in ten one volume had appeared. For four counties there were more than two volumes in print but the series was incomplete; in five nothing at all had been published.

The shift in sponsorship after the war also led to important changes in editorial policy. The initial plan had called for "general" volumes that were to begin with a geological introduction, followed by chronological sections on England up to 1066, a special article on Domesday Book, and then an ambitious series of topical chapters to include ecclesiastical, political, economic, and social matters, along with industry, arts, manufactures, architecture, and sports; schools were later added. To this was to be added a series of "special" volumes containing topographical accounts of each parish and manor in every hundred. In a small county like Bedford (completed by 1912) this meant one and a half "general" and two and a half topographical volumes. This format was adhered to fairly carefully until after the Second World War. The changes that were instituted then in volumes issued after about 1950 reflect stronger academic control of the series and the increasing interest in local history among professional historians. The topographical entries for each locality have been filled out substantially; the older scheme was limited to the descents of manors and advowsons plus a modest amount of general information. Much more attention is now given to the details of ownership, particularly those estates that were not technically manors, to physical growth, communications, and public services. Schools and non-Anglican religious bodies have been added to the topics included in each village entry.

Equally important are the changes that have been introduced into the "general" volumes. The earlier ones gave nearly a volume to geology with only a perfunctory survey of political and socioeconomic history. A postwar series, like the Wiltshire set, eliminates geology (although not general physical characteristics) and devotes a whole volume to the history of county government, including parlia-

mentary representation; a second is largely given over to county economic history; and a third is mostly concerned with the city of Salisbury. Ecclesiastical history is expanded from a catalogue of the monastic houses into a series of articles on medieval church history, the Church of England since 1542, Roman Catholicism, and Nonconformity.

These changes have left the original structure of the series basically untouched but have shifted emphases and added new kinds of information. The mildly antiquarian air that characterizes the earlier volumes is now replaced by a crisply professional tone. The kind of information provided is that which the twentieth century local historian requires to answer the wide-ranging questions he or she poses. The standards of scholarship are very high; articles are based heavily on archival material, both central and local. (The postwar creation of county record offices has made the researchers' task measurably easier.) Many of the articles are written by scholars of the highest distinction in their fields.

The current volumes reflect these differing aspects of the *History*. The Cheshire volume (the first for this county) is given over to the administrative history of the shire and to the history of its parliamentary representation, as well as that of Chester. This section is written by J. S. Morill, the author of a recent authoritative monograph on Cheshire in the Civil War. Volume 3 of Shropshire covers the same topics with a slightly different format. The city of Oxford is given a whole volume to itself; this provides an authoritative scholarly treatment of the entire history of the borough. A collective effort, organized on the standard format for borough histories, it is perhaps more a collection of articles on particular subjects than a coherent history. The borough of Stafford receives similar but much slighter treatment in a volume that also includes a survey of agriculture and separate accounts of the county's grammar schools and the University of Keele. The Yorkshire East Riding, Middlesex, and Somerset volumes are all given over entirely to topographical descriptions of local communities; *Roman Cambridgeshire* is the title of the volume for that county.

What estimate can we make of this great enterprise now in the eightieth year of its history? Its virtues are numerous, but they are offset by some flaws in execution, and more importantly by limitations, some of which are self-imposed but many more of which are inherent in the subject matter. The strength of the work is most apparent in the high quality of the postwar publications, marked by careful scholarship, built solidly on archival materials, and illuminated by a commendable breadth of vision. An offsetting weakness lies, of course, in the much narrower, more antiquarian, character of the prewar series. For those counties that were com-

pleted by the 1930s the deficiencies cannot be remedied; for the others, still incomplete, there is bound to be a very uneven quality to the finished county set.

The limitations of the project constitute a more complex problem. Some arise from the particular nature of English sources, especially for the earlier centuries; their legal and financial character will yield only certain kinds of information. Many urgent questions must go unanswered. Others derive from the announced purposes of the *History*. The review committee of 1944 put it succinctly in summing up the existing volumes as a "historical encyclopaedia of the English counties rather than an ordinary 'county history.'" Even after the revision of policy that followed on this committee's work, this statement remains fundamentally correct. The whole work continues to be more a great collection of reliable and relevant information, intelligently and helpfully organized, than a synthetic history of any of the English counties. Such a reservation does not denigrate the articles in the "general" volumes. The value to the historian of such a collection is hard to overestimate, particularly in the case of postwar publication. The quality is of the highest; their usefulness is limited by other considerations, which arise from the peculiar nature of English historical experience.

The special character of English local institutions poses problems for the would-be historian. Unlike many of the French provinces, most of the shires did not grow organically out of the experience of the countryside, of the *pays*, developing such indigenous characteristics as judicial or legislative bodies and local law. The English counties were from the tenth century on either created by royal fiat to serve the needs of the monarchy or adapted by the crown for these same purposes. Early on they became judicial and administrative units, shaped from above, uniform in character across most of the kingdom. One can write the history of such institutions as the shrievalty, the lieutenancy, or the commission of the peace more easily and profitably than that of Worcestershire or Rutland. Most counties never became political societies in any full sense, except for the special cases of parliamentary elections, and they lacked almost entirely distinctive legal or social institutions that would distinguish one from another. Nor do many counties correspond to definable agricultural regions. One can acquire a great deal of useful, dependable, but more or less random information about tenurial patterns or enclosure practices from the county histories, but clearly the history of agrarian institutions is better written in terms of natural farming regions (such as those proposed by Eric Kerridge or Joan Thirsk). Industrial history is probably best handled by the separate examination of coal mining, clothmaking,

and so on. The county is a more natural framework for the history of parliamentary elections (up to recent times). The history of the borough is more susceptible to this kind of treatment than that of the county; the legal and historical units are more likely to be identical. But even here the decline of many of the older boroughs after the Middle Ages and the emergence of new urban centers from the eighteenth century onward pose awkward problems.

The value of the *History* lies in the comprehensive and reliable collection of information. The historian who wishes to plot the distribution of Non-conformity or of Roman Catholicism will find such recent topographical works as volume 4 on Somerset invaluable. The editors have already done much of the work. Wherever the researcher seeks to collect data from widely scattered areas, spread over the country, the *History* is likely to prove useful.

But the fundamental flaw—the assumption that it is the county that is the characteristic unit of English local history—cannot be evaded. On the day—not too distant, we may hope—when this great enterprise is complete, scholarly historical study of many communities, larger and smaller, that have made up the complex fabric of English society will be measurably eased, both by the material made directly available and by the bibliographical clues that are included. But the purposes of the founders, so far as they aimed at a comprehensive understanding of English local history, will be only very partially realized.

WALLACE T. MACCAFFREY  
Harvard University

A. C. HEPBURN, editor. *Minorities in History*. (Historical Studies, number 12.) New York: St. Martin's Press. 1979. Pp. 251. \$27.50.

COLIN HOLMES, editor. *Immigrants and Minorities in British Society*. Boston: George Allen and Unwin. 1978. Pp. 208. \$27.50.

If not the most ethnocentric of the world's nationalities, Britons of the nineteenth and early twentieth centuries were certainly in the front rank. For many of them, "nigger" did indeed begin at Calais, and to others, as George VI is supposed to have said, "Abroad is awful." The British in the recent historical past were, despite a certain smug self-righteousness, at least as racist as their neighbors. Yet, until the post-World War II waves of nonwhite immigration, what might be called racially based opprobrium was in Britain itself reserved almost exclusively for the Irish. The unleashing of intense racial animosity toward immigrant nonwhites in recent years has caused British scholars to join the almost universal interest in ethnicity and national minorities. *Minorities in History* and *Immigrants and*

*Minorities in British Society*, the two books under review, are reflections of this trend and are collections of essays.

*Minorities in History*, edited by A. C. Hepburn, consists of papers read at the thirteenth Irish Conference of Historians held at the New University of Ulster in 1977. The net it aspires to cast is wide, but like all collections of conference papers, the book suffers from great diversity in the quality and the significance of the contributions. Sandwiched between a piece by the editor on the general theme of "Minorities in History" and A. E. Alcock's "A Reappraisal of Existing Theory and Practice in the Protection of Minorities," two essays that try to establish some unity and coherence for the enterprise, is a melange dealing almost entirely with Europe and North America. Other than an essay on "Indian Minorities Overseas" by T. G. Frazer, absolutely no attention is paid to Africa or Asia.

Given the few pages devoted to each subject, it is not surprising that essays dealing with limited themes are more effective than those painted with a broader brush. Thus, Alan Sharp's "Britain and the Protection of Minorities at the Peace Conference, 1919" and Alcock's "Three Case Studies in Minority Protection: South Tyrol, Cyprus and Quebec," come off better than, for instance, the less than fifteen pages devoted to "Huguenots in Seventeenth-Century France."

*Immigrants and Minorities in British Society* is edited by Colin Holmes, who is senior lecturer in economic and social history at the University of Sheffield. The book is divided into two sections. The first, general in character, consists of a short introduction by the editor bearing more or less the same title as the book and a longer, perceptive overview by V. G. Kiernan, entitled "Britons Old and New."

Part 2, which is the major portion of the volume, consists of a short piece on German immigration by Hermann Kellenbenz; an essay on English attitudes toward the Irish in Britain, 1860–1914, by Sheridan Gilley; a paper on the Chinese in Britain, 1860–1914, by J. P. May; a discussion of J. A. Hobson's antisemitism by Colin Holmes; and, finally, a fascinating analysis of what Nicholas Deakin, the author, terms the rejection of fascism and its racist overtones by the inhabitants of East London.

Despite the breadth of scope portended by its title, *Immigrants and Minorities in British Society* is a very limited book. Of the five studies in part 2, by far the longest is on "J. A. Hobson and the Jews," the presence of which can only be explained by the subject apparently being one of particular interest to the editor. For the rest, Gilley's essay on attitudes toward the Irish is well done; May's piece on the Chinese in Britain, although thought provoking, is too brief to do the subject justice. Clearly, *Immigrants and Minorities in British Society* is basically his-



torical in perspective; nevertheless, it is surprising that four of the essays deal to a significant extent with the Jewish minority while Indians, Pakistanis, and West Indians are totally ignored.

All in all, both books under review provide some useful new insights into minority problems in the world in general and Great Britain in particular, but the essentially eclectic and episodic approach mandated by multiple authorship lends itself to considerable triviality and little that is new or of genuine importance.

ROBERT A. HUTTENBACK  
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MICHAEL VAN CLEAVE ALEXANDER. *The First of the Tudors: A Study of Henry VII and His Reign*. Totowa, N.J.: Rowman and Littlefield. 1980. Pp. x, 280. \$22.50.

In *The First of the Tudors*, Michael Van Cleave Alexander provides an up-to-date history of the reign of Henry VII and attempts to provide an adequate biography of the king. Alexander indicates that his book is intended primarily for students and general readers, and they should find it easier reading than S. B. Chrimes's now standard *Henry VII* (1972) with its heavy concentration on rather difficult administrative and constitutional history. Specialists, however, should find Alexander's book a useful supplement to that of Chrimes. Alexander gives proper attention to important subjects slighted by Chrimes, notably Henry's foreign policy, his policy toward Ireland, his support of explorers, and his patronage of scholars and literary men. Add to this Alexander's sufficient and generally sound treatment of political, constitutional, administrative, fiscal, and economic and social aspects and we have a more balanced book. In the main, Alexander fairly presents both sides in handling matters of controversy; for instance, he convincingly refutes Chrimes's argument that there was no political motivation involved in Henry's delaying his marriage to Elizabeth of York for some four months after Bosworth. He is one-sided, however, on the most contested issue of all, presenting only evidence that the king was rapacious in his last years.

Unlike his predecessors, Alexander had available to him the works of C. D. Ross on Edward IV, which indicate that Henry VII deserves a higher rating than his father-in-law. Alexander has obviously been influenced by Ross. His Henry is no mere continuator of Edward's policies; rather, the first Tudor is credited with completing the work of the first Yorkist, that is, "revitalizing the monarchy and restoring the rule of law" (p. 1). Henry was no great innovator who created a "new monarchy"; rather, he was a truly successful ruler who left Eng-

land and the English better off at his death than they had been at his accession. Alexander offers much to support his appraisal of Henry VII, and he has succeeded better than anyone to date in using the known evidence to bring to life the king as a ruler and a public figure. He joins his predecessors, however, in failing to bring to life Henry the person. He has only been able to present fleeting glimpses of the king's personal relationships with his family, friends, and acquaintances. It is more than likely that Henry was close to his mother, but this is not proved by two letters in which Lady Margaret Beaufort respectively addresses her son as "my own sweet and dear king" and "my . . . only beloved son" (p. 167). Of course, the well-known story of how Henry and Elizabeth of York tried to console each other when they learned of Prince Arthur's death is touching; unfortunately it is also unique. Isn't it time to admit that the personality of Henry VII is too ill-documented to allow a biography in the full sense of the word?

MORTIMER LEVINE  
*West Virginia University*

GILBERT JOHN MILLAR. *Tudor Mercenaries and Auxiliaries, 1485-1547*. Charlottesville: University Press of Virginia. 1980. Pp. xv, 223. \$13.95.

Historians of Tudor and Stuart England have tended to steer clear of dealing with military affairs. Gilbert John Millar's enterprising and important study concentrates upon a subject that, with the honorable exception of the work of C. G. Cruickshank, has been largely ignored. Millar states in his preface that the idea behind his own research originated from a single footnote in W. K. Jordan's biography of Edward VI.

*Tudor Mercenaries and Auxiliaries, 1485-1547* goes some way toward redressing this imbalance. Building upon Cruickshank's three volumes, Millar introduces his readers to the history of the early Tudor army before proceeding to discuss the role of the mercenary in sixteenth-century European warfare as well as the "soldier trade" in general. After this essential groundwork, the major portion of the book deals with the last French war of Henry VIII and the temporary occupation of Boulogne. A short, concluding section looks briefly at the employment of foreign soldiers in England and on the Scottish border between 1547 and 1558.

Within this format, Millar accomplishes two major tasks. First he provides a detailed account of the English campaigns in northern France that is valuable in its own right. In addition, he uses the fighting in the Boulonnais to investigate how the mercenaries and auxiliaries were recruited, mustered, organized, and paid. The overall impression is that they were unscrupulous scoundrels, delighted to ex-



tract as much money as possible from hiring agents but a little less eager to appear in the field and risk their lives. Reliance upon troops like these was a recipe for protracted and indecisive warfare. Not only were the hirelings of limited martial use in France, but they also showed themselves to be equally unreliable in suppressing rebellions or keeping order within England itself. When they were ordered to march against the Scots, Millar tells us, they behaved so disgracefully in Durham and Newcastle-upon-Tyne that Henry was obliged to ship them back across the English Channel.

At times this book gives the impression of a long essay that has been stretched into a more substantial work. The chapter on the causes of the French war adds little to existing knowledge and could easily have been accommodated in less space. On the other hand, this reviewer would have welcomed more attention to the European mercenary trade in the first half of the sixteenth century and to the employment of hired soldiers in England, particularly during the reigns of Edward VI and Mary. Nevertheless, Millar has produced a well-written and worthwhile study that navigates a course through uncharted seas.

JOHN CHILDS  
University of Leeds

JACOB M. PRICE. *Capital and Credit in British Overseas Trade: The View from the Chesapeake, 1700-1776*. Cambridge: Harvard University Press. 1980. Pp. viii, 233. \$18.50.

Much capital and credit were needed for the production, shipping, and marketing of agricultural staples of the American and West Indian colonies. Finance was needed not only to tide the planter over the growing season but also to cover the gap in time between the shipment of European goods to the colony and the sale of staples in British and Continental markets. In this thoroughly researched and ably written work, Jacob M. Price finds that the Chesapeake trade was noteworthy in the eighteenth century for the long credits it required; it was, he says, "disproportionately capital-intensive and credit-prone" (p. 3). Price's purpose is to ascertain the various sources of capital and credit in Britain, how the merchants trading to the Chesapeake were able to sustain the high volume of credit, how credit contributed to economic growth in the colonies and metropolis, and how the credit system was pushed to the point of collapse with economic and political consequences that exacerbated the already strained relations between the colonies and the mother country.

Not only were British merchant houses important sources of financial supply for the colonies, but they

also tapped the resources of other middlemen and bankers. Capital for the Chesapeake trade came initially from the personal fortunes of merchants who formed partnerships varying from two to three partners in London and from four to ten or twelve partners in Glasgow. In addition to their own capital, merchants borrowed from individuals and banks on the security of bonds or notes. But the major suppliers of credit were the large wholesale linen-draperies and ironmongers of London and the larger outports that supplied goods on twelve months' credit and allowed a discount for early payment. Price contends that this long-credit export system was a peculiarity of British commercial organization that was not characteristic of Continental countries.

Given the favorable conditions that obtained in the metropolis and colonies in the decades before the American Revolution, it is not surprising that the credit system developed a momentum of its own. Fearful liquidity problems triggered a crisis in 1772-73, however. Hardest hit were members of the indigenous merchant class in the tobacco-growing regions who owed large debts to British merchants who had supplied goods on long credits in what was termed the "cargo trade." Rather than dismissing the political repercussions of the debt crisis, Price believes that "the economic distress that followed the crisis must have heightened the sense of unease fed from so many other sources" (p. 137). Indeed, he finds that the merchants who had formerly carried on the cargo trade were much more revolutionary than their counterparts north of the Chesapeake region.

Price's contribution is to show how British capital and credit were mobilized for the Chesapeake trade, to sketch the careers of leading actors, to describe the institutions they developed, and to show how capital and credit contributed to the performance of the Atlantic economy. No review of this length can do justice to the author's command of sources, ingenious use of quantitative and literary data, maturity of judgment and insight, and ability to analyze and interpret the data. It is hoped that Price will continue his fascinating studies of Britain's money markets and overseas trade and perhaps expand the scope of his studies to include the African slave trade and the staple trades in rice, indigo, and sugar.

RICHARD B. SHERIDAN  
University of Kansas

MICHAEL FREEMAN. *Edmund Burke and the Critique of Political Radicalism*. Chicago: University of Chicago Press. 1980. Pp. 250. \$21.00.

Because he is candid and sees both the strengths and weaknesses, Michael Freeman succeeds in or-

ganizing Burke's political thought, widely conceived, from the components in writings produced over some forty years as responses to particular problems Burke encountered in his public life. Although he never attempted a systematic theoretical treatise, certain constants guided Burke's reflections upon events. The greatest of these events was the French Revolution, which Burke called a "complete revolution" (p. 219). His writings of the 1790s are especially important because Freeman is concerned with the "scientific and moral problems raised by modern revolutions" (p. 6).

Intending to be systematic, Freeman begins with Burke's natural law, metaphysics, and empirical epistemology. Conventional for Burke's time, his philosophy, especially his "epistemological populism" (p. 30), includes conservative elements of tradition and prescription, and it implies utility and even political democracy. Although his philosophy is the "basis for his political theory," it does not lead inexorably to "conservative conclusions" and thus "it fails" (p. 34). Burke's moral theory is compatible with his metaphysics and, like the latter, is grounded in religious faith. Freeman says it is sounder as psychology than as philosophy because, if not always logically demonstrable, it takes account of mundane life. Yet life and therefore knowledge of society are uncertain; society is complex and mysterious. Commonwealths being "moral essences," there can be no "deterministic" social laws even though, not quite the same thing, there are "grand eternal laws governing social life" (p. 55). These derive from understanding of the moral nature of man and the circumstances in which society exists, that is, from history. This appreciation informs Burke's conservatism and his hostility to speculative radical theorists such as he identified with the French Revolution.

When he considers theories of government and political change and their particular subdivisions, Freeman is on well-trodden ground. But here and there are some arresting discussions. For example, unlike some modern commentators, Freeman thinks Burke has been undervalued as a theorist of revolution. Freeman constructs from Burke's expressions an ordered theory and analysis of revolution. He refers mainly to the greatest one, the French, and to the reforming, constructive ones, the American Revolution and the Revolution of 1688. His treatment of Burke's understanding of the causes of the French Revolution is fresh and provocative and reduces the importance of conspiracy theory. Freeman is more concerned with Burke's sociology. This concern guides his analysis of Burke's discussions of the dynamics and consequences of revolution. All of this relates implicitly and explicitly to political radicalism, particularly that of the extreme variety. Freeman is impartial; his sympathy for Burke is

built on understanding, and that enables him to see points of inconsistency or even failure in some of Burke's arguments. Earlier controversy about Burke has cooled off; this book might revive it and supply some new points for further discussion.

CARL B. CONE

University of Kentucky

SUSANNA WADE MARTINS. *A Great Estate at Work: The Holkham Estate and Its Inhabitants in the Nineteenth Century*. New York: Cambridge University Press. 1980. Pp. xiv, 289. \$54.50.

Thomas William Coke of Holkham, later the earl of Leicester, was one of the leading personalities of the agricultural revolution. Susanna Wade Martins's book is important both as a study of Coke's showpiece estate and as an evaluation of farming practices in arable England during the agricultural revolution, the period of high farming, and the subsequent depression.

The first chapter describes the 40,000-acre estate within its Norfolk setting. Chapter 2 discusses the landlords, two of them spanning the years from 1776 to 1907, in itself a singular demographic achievement for father and son, and not without significance for the stability of the estate administration. Chapter 3 explains this administration and analyzes income and expenditure. It reveals Coke's indebtedness up to 1822, followed by a long period of cheerful reliance on estate profits that ended in the 1870s, when his son diversified the family's financial interests by investing heavily in overseas railways.

Chapter 4 is devoted to the tenants—some of them leading agriculturalists in their own right—and to their farms. This 80-page chapter is one of the outstandingly successful features of the book and is replete with maps, plans, and photographs. We see the reorganization years up to 1840 typified by new farmsteads centrally located within their fields, followed by additions and adaptations as the emphasis swings from cereal monoculture toward mixed farming with many beef cattle. Martins writes with so light a touch about such intrinsically dull subjects as the preservation of manure that one must conclude she has both poetry and farming in her blood!

The penultimate chapter brings the laborers, their cottages, religion, and education into the picture. The Cokes were benign despots, neither inhibiting Nonconformity nor exercising overpowering control over schools, except in the home village of Holkham, which was only one of a score of villages on the estate. Several hundred cottages were let direct to laborers, thus heading off the less enlightened despotism of tenant farmers. Perhaps this kind of social policy is to be expected from Coke, in view

of his liberal political views, including opposition to the American War of Independence. The last chapter rounds out the study by summarizing the impact of the estate on the landscape.

This reviewer has few criticisms. One or two maps lack clarity, and there are a noticeable number of misprints. A parish-by-parish analysis of estate property would have been useful. Although M. A. Havinden's *Estate Villages* (1966), a study of Lord Wantage's Berkshire estate and one of the few parallel studies, is cited in the bibliography, it has not been used for explicit comparison. But these are small blemishes on a magnificent achievement.

DENNIS MILLS  
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HUGH CUNNINGHAM. *Leisure in the Industrial Revolution, 1750-1880*. New York: St. Martin's Press. 1980. Pp. 222. \$25.00.

This book makes two major contributions. First, it presents overwhelming evidence that the history of popular leisure cannot be interpreted simply as the repression from above of old customs, as the creation of a recreational vacuum that would be filled by late nineteenth-century entrepreneurs of leisure. To the contrary, popular culture in the nineteenth century "made itself" in complex interaction with work, with reform tendencies within and without the working class, with changes in social geography, with politics, and of course with an older, but by no means static, leisure culture.

Second, Hugh Cunningham opens up the history of a new discourse. "Leisure" is shown to be a creation of the industrial revolution; in contrast to the older "idleness," it came to be viewed as being more than an abiding seed ground of sin and disorder. It was transformed into "recreation," which has meaning only in relationship to new forms of work, and it added cultural baggage. Recreation could be "rational" or "irrational," and leisure as recreation could be at once an agency of class harmony, as in the democracy of mass spectator sports, and of class isolation, as in the whole host of activities in which middle and working classes were sharply segregated.

Thus, the history of leisure discourse is one of discontinuity. But Cunningham shows that the history of leisure activities themselves is one of considerable continuity. Certain old sports like boxing and horse racing enjoyed their greatest popularity during the industrial revolution. Railroads made these as well as the pleasures of the seashore and of large regional fairs more widely available. So-called traditional customs were not traditional at all; genuine tradition, Cunningham suggests, had been destroyed by the Reformation. More recently each trade and lo-

cality had developed patterns of work and leisure that came to be defended, in some cases successfully, as "customary." By the middle of the nineteenth century, in heavily mechanized industries, such customs had lost their meaning, thus providing a niche in the popular cultural ecology for a new commercialized leisure. In other trades, however, old customs continued and "new customs" were created throughout the period. There was thus never a leisure vacuum as some have maintained.

Cunningham does not deny that middle-class evangelicals and entrepreneurs, each for their own reasons, attacked old habits of work and leisure. But he maintains that working-class secular as well as religious radical traditions also opposed, for example, blood sports and the culture of drink and sought to substitute for them less physical and more cerebral forms of recreation. He is quick to point out that popular leisure was not only affected by, but also affected, the leisure of the middle classes. Music halls, cricket, and football were all examples of the upward percolation of culture. Changes in popular culture thus came from within as well as from outside the working class and influenced as well as were influenced by changes in bourgeois culture.

This book is a model of the new social history. The details of the everyday life are prisms through which important historical patterns are refracted. The battle over rapidly disappearing urban space is analyzed, for example, in the context of Sheffield and London boys who, because of prudery and crowding, lose the opportunity of swimming nude in canals and waterways and of east Londoners in the second half of the nineteenth century who tore down enclosure fences in Epping Forest and successfully fought their convictions in court with the aid of the middle-class Commons Preservation Society.

The book's failings are largely due to the relative neglect of the field by historians. We simply do not know enough yet to generalize about the role of leisure in the lives of women, about the play of the young or the leisure activities of the old, or, in a detailed way, about the relationship of work and leisure. We also know less than we should about middle-class leisure, but here the fault is less paucity of data than lack of interest.

Cunningham's concern and that of most historians has been either in the history of aristocratic sport or in the interplay of popular, that is, working-class, leisure and the forces that shaped and were shaped by it. Thus concert halls, art galleries, and resorts appear only insofar as they excluded or incorporated the great unwashed, while card playing and reading, not to speak of such leisure time activities as charity, church work, and the performance of civic duties, are entirely absent. In short, the

history of what Trollope called the richest and largest leisure class in world history, the nineteenth-century English bourgeoisie, still needs to be written.

This book, however, adds new complexity and richness to the history of leisure. It becomes in Cunningham's analysis a significant aspect of the economic, political, and cultural history of the nineteenth century.

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FRANCES FINNEGAN. *Poverty and Prostitution: A Study of Victorian Prostitutes in York*. New York: Cambridge University Press. 1979. Pp. x, 231. \$22.50.

Frances Finnegan writes with a corrective zeal. An important part of her purpose in this book is to dispose of the errors of previous writers in the field. Were this merely a matter of empirical knockabout, simply a question of contradictory data, then it would have no great significance. On the contrary, however, it turns on a fundamental question of methodology. Previous work on Victorian prostitution, she contends, has swallowed whole the assumptions, both ideological and psychological, of contemporary Victorian commentators in general, and William Acton in particular. Such work has reproduced his emphasis on institutions, on the sexuality of the bourgeois Victorian male, and has for the most part neglected the material and other circumstances of prostitutes themselves in favor of a reliance on official documentation. In these contentions Finnegan is for the most part correct. Accordingly, *Poverty and Prostitution* seeks to reconstruct the material world, experiences, and circumstances of some 1,400 prostitutes and brothel-keepers in the city of York over a fifty-year period. It is based on a thorough and detailed examination of a wide range of historical documents, including newspapers, police records, and census materials among much else. In short, the book is a successful attempt to subject the matter of Victorian prostitution to the now familiar and honorable methods of "history from below."

This results in an account that is partly statistical in a loose sense, an attempt to elucidate the aggregate characteristics of the York prostitute population. Partly the account rests on the extensive reconstruction of the biographies of many women who, by careful methods of nominal tracing, may be found in various classes of historical records. Sensitively deploying a wide range of difficult, if not intractable data, the book deals in assiduous detail with the conduct of prostitution in Victorian York and its geographical distribution; relations between prostitutes and brothel-keepers; the social character of the clientele; and finally with "drink, destitution, disease" and "rescue and reform."

It is in the closing chapters that Finnegan clinches her argument concerning the biases and ulterior motives in play in Victorian accounts of prostitution. For example, she overturns the assertion of William Acton that few prostitutes so quickly found the ugly death or the demoralized destitution suggested by Victorian popular fiction. For, as she successfully shows, the history of prostitution in York is a history of poverty, both as the causal circumstance of the prostitute's recruitment and as a near inevitable consequence of her occupation. On this account the abiding condition of prostitutes in Victorian York was one of an insecure poverty leading often to total destitution.

It is merely a cavil to suggest that Finnegan's book suffers much of the methodological elision characteristic of "history from below"—notably that of a wider account of the economy and class structure of Victorian York, the structural context of prostitution. Rather she is to be congratulated on bringing the difficult evidences of the subject into the disciplined orbit of that methodology.

KEITH NIELD  
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JUDITH R. WALKOWITZ. *Prostitution and Victorian Society: Women, Class, and the State*. New York: Cambridge University Press. 1980. Pp. ix, 347. \$22.00.

The Contagious Diseases Acts of 1864, 1866, and 1869, legislated to prevent the spread of venereal disease among enlisted men in garrison towns and ports, attempted to control infection among "common prostitutes" by identifying them and subjecting them to fortnightly internal examinations by government doctors. Male clients were left entirely free of this health and police supervision. The C. D. Acts, the public controversy over them, and the organized campaigns to repeal them (accomplished in 1886) have been the subject of contemporary accounts and recent scholarly studies, but Judith R. Walkowitz's book is by far the best and most important because of its broad social context. Walkowitz not only explains the timing of the acts—how, why, and where they were enforced, and who conducted repeal campaigns—but also analyzes more basic questions such as "the enthusiasm for state intervention" into the lives of the unrespectable poor, "both prostitutes and enlisted men . . . of the social 'residuum,' the casual laboring poor who inhabited the 'nether regions' of society" (p. 3). She evaluates the legislation as instrumental in crystallizing and shaping social attitudes and beliefs about "fallen women" and the outcast social classes. She explodes the notion of a unitary Victorian culture perpetuating a single sex code, showing that several subcultures simultaneously followed distinct patterns of Victorian sexuality.



Part 1 focuses on the contemporary view of prostitution and includes not only a social profile of the common prostitute but also specifics based upon manuscript census records, police columns in local newspapers, and institutional files. It explores the social and ideological context of "the Great Social Evil" and its alleged threat of sin, disease, and contagion. Part 2 anatomizes the C. D. legislation, its regulationists, and also the "repealers"—the latter including the incipient feminist Ladies' National Association. Victorian class and gender conflict are exposed. Part 3 offers two illustrative cases under the acts, Plymouth and Southampton, and reveals the making of an "outcast group" from prostitutes and working women. The operation of hospitals and the tactics of repealers are studied on the local level.

Walkowitz reveals the complexity of the social, economic, moral, religious, and political (including feminist) issues that were entwined in the C. D. controversy through a virtuoso analysis. Not the least contribution is an epilogue that deflates the notion of triumph in repeal. This chapter discloses the rise of "purity crusades" promoting chastity and social hygiene for all, and repressive measures against the same women who had been controlled and brutalized under the C. D. Acts. Thus the nineteenth-century controversies over sex, disease, vice, and regulation yielded no positive assertions of female sexuality. "Separate spheres" ideology persisted. Walkowitz exposes the void that still exists in the historical explanation of changes in ideas and policies on sexual and social relations of the sexes that have transpired since late Victorian times. The exposure is a worthy challenge to a social historian who can match Walkowitz's gifts in research and analysis.

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PATRICK JOYCE. *Work, Society, and Politics: The Culture of the Factory in Later Victorian England*. New Brunswick, N.J.: Rutgers University Press. 1980. Pp. xxv, 356. \$25.00.

Patrick Joyce's fascinating study of the factory towns of the north of England may be less original in its implications than he would like us to believe, but it remains an important and often persuasive evocation of the culture and the controls of these mill communities. The argument pivots around a continuing contrast between the cotton towns of Lancashire and the woolen and worsted districts on the other side of the Pennines. By mid-century the Lancashire cotton industry had developed a much higher level of factory mechanization and a greater elimination of the skills and culture of the artisan than had the West Riding. With these changes

came the mill towns, those introverted communities of employers and workers tied together in a culture of deference and paternalism that generated a new social harmony. Here Joyce finds his great paradox. Rather than factory mechanization producing social disruption, its full flowering in Lancashire produced a stability never achieved in the West Riding, where the struggle for control of the work process stimulated a radicalism that in the cotton towns had died with Chartism.

The paradox is a neat one, and Joyce uses it well, turning it and re-examining it as he investigates the social and political relations of his mill towns. Paternalism and deference emerge as living forces that enabled the whole of this working class—and not just an elite of it—to be captured for the existing order. The all-dominant employer controlled the work, the politics, and the religious sectarianism of his work people and thus "made their acceptance of the social regime of capitalist industry a matter of inward emotion as much as of outward calculation" (pp. xiii–xiv). The resulting clan-like loyalties to the politics of the employer were an expression of culture and not of political consciousness, and they only began to decline toward the end of the century.

Such a brief summary serves badly a rich argument, and the central chapters on the mill community, above all, are fine pieces of historical re-creation. Joyce's strength, however, lies not in the detailed development of a point with evidence and argument but in the adjustment and reformulation of that argument, the drawing out of its implications. One result is that much of the book makes awkward reading. Another is a lack of specificity that belies the wealth of local detail. Joyce rarely sets out systematically to demonstrate his case; rather, he illustrates it, and at crucial stages of the argument—such as the existence of communities and the content of deference—that is frustrating. Equally so is the lack of serious attention to those craftsmen, shopkeepers, and the like who constituted the forces of independence in these communities.

The most serious problem with the book is in Joyce's claim that the experience of Lancashire can serve as the basis for reinterpretation of later Victorian social relations. After all, even the cities of Manchester, Liverpool, and Leeds cannot, he tells us, be incorporated in his picture. They are too diverse, and thus contain those forces for political and cultural independence so important in most urban areas. Yet it is to Joyce's credit that he is always seeking to press the wider implications of his argument. The result is an uneven but evocative, lively, and always stimulating book.

GEOFFREY CROSSICK  
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A. G. L. SHAW. *Sir George Arthur, Bart, 1784-1854: Superintendent of British Honduras, Lieutenant-Governor of Van Diemen's Land and of Upper Canada, Governor of the Bombay Presidency*. Carlton, Australia: Melbourne University Press; distributed by ISBS, Forest Grove, Oreg. 1980. Pp. xv, 307. \$34.50.

A. G. L. Shaw concludes this scholarly and judicious study of a fascinating and formerly maligned colonial servant with the statement, "Arthur, unlike most of his contemporaries, made colonial government a life-long career, before this became the normal practice of the Empire's administrators" (p. 284). Arthur held four posts: superintendent of British Honduras, lieutenant-governor of Van Diemen's Land and later of Upper Canada, and finally governor of the Bombay Presidency. He is a biographer's dream, a workaholic who pushed his pen from ten to fourteen hours a day all his life. Shaw comes well equipped to harvest this field, having first published articles, then a superb study of the convict system (*Convicts and the Colonies* [1966]), and finally a one-volume edition of John West's famous *History of Tasmania*. The writing of other scholarly books gave him further insights into the working of the nineteenth-century British empire. No longer the harsh, iniquitous, and ruinous monster of Tasmania, Arthur emerges from this study as an honest, autocratic, efficient, and canny bureaucrat whose sole object was to keep his masters in the Colonial Office satisfied and to accumulate money to support his five sons, seven daughters, and an ailing wife. Shaw is too sophisticated a scholar to dabble in psychohistory without sufficient evidence. Arthur does appear enigmatic and goaded by a religious strain that found relief in a compulsive, even manic, chain to his desk. A slave to duty, he was a "devout and convinced evangelical, with a truly Calvinist belief in justification by faith" (p. 22) and was a friend of Stephen, Buxton, and Wilberforce.

Shaw studied the enormous corpus of documentary material in England, Belize, Hobart, Toronto, and Bombay. A little less than half the book covers Arthur's administration of Van Diemen's Land (1824-36). All kinds of fascinating details appear. Arthur's careful accounting notes reveal that he became a very wealthy man in Hobart both through personal loans at prevailing rates of interest and by shrewd purchases of land. Shaw provides some new insights into the terrible "Black War" between convict and aborigine. We learn that Arthur was eternally jealous of his authority; this continued in Upper Canada where he felt humiliated to be under the "imperious and impulsive" Durham as governor-general and later under Charles Poulett Thomson, Lord Sydenham. Arthur did not like the idea of responsible government and said so. Yet he was a great improvement over his predecessor, Sir Francis

Bond Head—known as "Bone Head" by many. Although incomplete, Shaw's description and assessment of Arthur's work in Bombay are more detailed than in any other study. Possibly this section had to be cut, as the book is tightly compressed.

Such compression apparently caused an inadequate index, but, *mirabile dictu*, the notes are at the foot of the page, the bibliography is organized into seven sections (including the four postings), and the illustrations are excellent. Arthur emerges as a ruthlessly firm administrator with a total mastery of detail. He was an unswervingly loyal servant of the crown yet a fussy, bigoted, although high-minded man, who managed all his life to command the respect of his superiors in Whitehall and to convince them of his position through many quarrels large and small. Shaw might have given short analyses of where his views differ from those of earlier revisionist biographers such as W. D. Forsyth, M. C. I. Levy, and especially from Manning Clark's long description and assessment of Arthur in the second volume of his *History of Australia* (1968). Such cavils should not detract from a superior piece of scholarship.

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R. W. LISCOMBE. *William Wilkins, 1778-1839*. New York: Cambridge University Press. 1980. Pp. xv, 297. \$49.50.

The Greek Revival was never really popular in England. In Scotland, Germany, and the United States, the geometrical precision and austerity of neoclassicism was adopted as a national style and resulted in some memorable monuments, but in England neo-Greek was generally thought to be cold and dull. It often was. Smirke's pedantic British Museum is redeemed by its massive and appropriate gravity, but there is rather less to be said in favor of Wilkins's buildings. William Wilkins, indeed, was largely responsible for bringing the Greek Revival into disrepute in England, and so encouraged the alternatives: the Italianate of Barry, the rich sculptural Roman grandeur of Cockerell and Elmes, and the picturesque Romanticism of the Gothic Revival. Pugin wordlessly ridiculed Wilkins by drawing the National Gallery on the frontispiece of *Contrasts*—it was self-evidently ludicrous.

Yet Wilkins enjoyed a brilliant career. Born in 1778, his education at Cambridge coincided with the height of the fashionable enthusiasm for things Grecian. His earnest classical studies and his trip to Greece in 1801-03 brought him into contact with influential philhellenes, such as Lord George Gordon, later earl of Aberdeen. Wilkins became a Fel-

low of Gonville and Caius, but he also followed his father in the profession of architecture. Chiefly through the lobbying of Thomas Hope, Wilkins displaced Wyatt in 1806 as architect of the first new college to be founded in Cambridge since the Reformation. Downing College should have been his triumph, but his Greek Revival design remains incomplete—although the “campus” plan anticipated Jefferson’s at Charlottesville. Ironically, Wilkins’s best Cambridge works are not Grecian but Gothic: he designed new courts at Trinity and Corpus and his open stone Perpendicular screen along King’s Parade, joining King’s Chapel with his new buildings, is a felicitous triumph and a much-loved landmark.

Unfortunately, R. W. Liscombe presses his subject’s claims too far. Although never truly original, Wilkins certainly had his moments: the famous, colossal brick and stucco Doric portico at the Grange is a fine thing—at a distance; the great steps and Corinthian portico at University College, London, are splendid—even if spoiled by the half-hidden shy dome behind; and it is Dublin’s loss that the I.R.A. blew up his Nelson Pillar (a fact not mentioned by the author). But it will not do to blame Wilkins’s albeit difficult and mean clients for the miserable failure of his most important job: the National Gallery. Government stinginess, at a time of political and social turmoil (1832), scarcely excuses the indecisive staccato facade, and the feeble pimples of domes were a waste of money and best omitted. Think what Schinkel would have done with a site overlooking Trafalgar Square!

Liscombe’s biography is painstaking and comprehensively documented; there will be no need for any other study of William Wilkins.

GAVIN STAMP  
London, England

THOMAS L. HANKINS. *Sir William Rowan Hamilton*. Baltimore: Johns Hopkins University Press. 1980. Pp. xxi, 474. \$32.50.

This “intellectual biography” both supplements and illuminates Robert Graves’s massive three-volume *Life of Sir William Rowan Hamilton* (1882–89). Thomas L. Hankins wants us to understand the development of Hamilton’s ideas. He believes that to appreciate Hamilton’s works, we must realize their full context. Hankins takes great care to reveal, and then to document, the specific connections between Hamilton’s mathematical and scientific works and his private life, metaphysics, poetry, and idealist philosophy.

Hamilton’s early intellectual development was shaped by his affinity for Kantian philosophy and

by his exposure to the French mathematical tradition. He devoted himself to highly abstract and general mathematics, caring little for physics or for practical applications. Hankins divides Hamilton’s scientific career into three main sections that overlap chronologically: his work in geometrical and physical optics, his interest in dynamics and an optical-mechanical analogy, and his interest in algebra leading to quaternions and the creation of new algebras. Chapters on these developments are separated by chapters on Hamilton’s personal life as Irish Astronomer Royal and chapters that document his enthusiasm for poetry and philosophy. Hamilton enjoyed broad scientific and social relationships. He met or corresponded with virtually every important British scientist and mathematician, and he discussed poetry and philosophy with his friends Wordsworth and Coleridge.

Hankins’s treatment of Hamilton’s works is always clear and concise. His biography displays a mastery of the technical details of Hamilton’s mathematics as well as an understanding of the origins of Hamilton’s ideas. Hamilton’s first important works were in optics. They coincided with the British debate over wave versus corpuscular optics and with the formation of the British Association for the Advancement of Science in 1831. Hamilton’s very general treatment of geometrical optics and his theoretical prediction of conical refraction established his reputation and made him a prominent member of the B.A.A.S. His interest in optics led to an interest in dynamics and the hope of uniting optics and mechanics using very generalized mathematics. Hamilton’s dynamics never had practical impact in the nineteenth century, but Hankins demonstrates how Hamilton’s optical-mechanical analogy served Erwin Schrödinger in his development of wave mechanics in the 1920s.

Hamilton’s sudden discovery of the quaternion formulas (while strolling along a Dublin canal with his wife) in 1843 opened the way for modern algebras. Hamilton’s developing interest in algebra in the 1830s was shaped by his reading of Kant. His “Essay on Algebra as the Science of Pure Time” (1835) shows this link. His new quaternions occupied him from 1844 until his death in 1865. Hamilton was uninterested in the application of quaternions to physics, but he was sensitive to their geometrical significance. Hankins shows how the invention of vector analysis by J. W. Gibbs and Oliver Heaviside depended upon Hamilton’s quaternions, after his death.

This biography is extraordinarily well written and a pleasure to read. It does not neglect the broad context of nineteenth-century British life. We see the “Irish question,” the potato famine, the organization of science, the Oxford movement, and the Romantic movement all reflected in Hamilton’s

life. All historians interested in the nineteenth century may profit from this scholarly work.

HENRY JOHN STEFFENS  
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CHUSHICHI TSUZUKI. *Edward Carpenter, 1844-1929: Prophet of Human Fellowship*. New York: Cambridge University Press. 1980. Pp. x, 237. \$34.50.

Distinguished biographer of H. M. Hyndman and Eleanor Marx, Chushichi Tsuzuki has now turned his attention to Edward Carpenter. Carpenter is perhaps best remembered as the composer of *England Arise!*—one of British socialism's most popular tunes. But the student of the socialist revival of the 1880s and the origins of the Labour party in 1900 continually encounters this prolific author, poet, pamphleteer, and activist. During a long life that began in 1844 and terminated in 1929, Carpenter never ceased to struggle for a better world.

A kindly, generous human being dedicated to the construction of a society in which all men and women might achieve physical and spiritual happiness, Carpenter could easily have been regarded as a monster by the bourgeois society that had nurtured him. Brought up in a comfortable Brighton home, he attended Cambridge University and seemed destined for an ecclesiastical career. Influenced by Christian Socialism and the writings of Walt Whitman, however, Carpenter abandoned his clerical attachments in 1874. He became an extension service lecturer and then, in 1877, made a pilgrimage to Whitman's home in New Jersey.

Returning to England, a personal crisis culminated in a decision to purchase some land at Millthorpe, near Sheffield, where Carpenter turned to market gardening. His farming did not impede his restless political activity. Carpenter had become convinced that the acquisitive values of capitalist society were stifling all that was human and creative in the individual. Hypocrisy was everywhere. The well-being of the few was paid for by the suffering of the many. Community and the common good must be rediscovered; this led Carpenter to admire the natural joy of the savage, which he identified with the working class and not with the repressed bourgeoisie. Men and women must be treated equally.

Although a socialist by the 1880s, Carpenter disliked the acrimony that often separated H. M. Hyndman, William Morris, the Fabians, the trade unionists, and the anarchists. A single unified movement held little attraction for Carpenter; he found strength in variety and sympathized with anarchists as well as socialists. He was an irritant, however, to George Bernard Shaw, who regarded Carpenter's fuzzy views about noble savages as positively harmful.

Not content with advocating a socialist millennium, Carpenter joined movements for hospital and prison reform and supported campaigns against smoke pollution, vivisection, and cruel sports. He enthusiastically endorsed trade unionist activities and attempts to organize a labor party. An interest in Hinduism and Eastern mysticism brought him to Ceylon and India. During the 1890s Carpenter also became more aggressive in defense of his homosexuality. Opposing the views of Havelock Ellis, Carpenter argued that homosexuality was normal and healthy; he submitted an anonymous evaluation of himself for Ellis's pioneering sexual studies.

Tsuzuki has made very effective use of the Carpenter papers at the Sheffield City Library. Interesting letters were also exchanged with such literary figures as Olive Schreiner, Siegfried Sassoon, Robert Graves, and E. M. Forster. Tsuzuki concludes this fine biography of a fascinating human being with the observation that Carpenter's "homosexuality, though part of his essence, was by no means the sole reason for his becoming a Socialist, but in him Socialism and sex reform were closely interrelated, and from these, he felt, would arise a new sentiment of humanity" (p. 199).

THOMAS J. SPINNER, JR.  
*University of Vermont*

EDWARD M. SPIERS. *Haldane: An Army Reformer*. Edinburgh: Edinburgh University Press; distributed by Columbia University Press, New York. 1980. Pp. vi, 240. \$21.00.

Later in life, Lord Haldane contended that Britain's new Continental strategy had provided the primary framework for his structuring of the Expeditionary Force. Edward M. Spiers argues instead that the nature of the British army reform from 1906 to 1912 was dictated by the need to reduce the army estimates. Haldane, who disliked the excessive military spending habits of the Unionists and had to placate the radical elements of his own Liberal party, placed a spending limit on the army as soon as he entered the War Office. Thereafter, all army reform had to live within these financial limits. Continental strategy did help to shape the auxiliary forces into the Territorials, whose men would be needed should Britain enter a major war.

Spiers examines the political problems and vested military interests that Haldane had to contend with in reforming an army whose shortcomings had been so openly displayed during the South African War. When Haldane entered the War Office in 1905, he was already an adherent of the "blue water" school, voluntary recruiting, and efficiency, but as a member of the opposition he had been markedly dis-

interested in any detailed study of the army. Yet, while Haldane relied upon some close personal advisers, Spiers argues—not always convincingly—that the war secretary usually put his own stamp on plans. Haldane proved an adept if boring parliamentary leader.

Normally decisive and determined, Haldane did compromise on the structure of the Territorial Army because of pressure from vested interests. He contributed little to the development of the Imperial General Staff; indeed, before entering office he had ignored the “staff” concept and had looked for “one good man.” Haldane lost interest in planning for mobilization and failed to gain a sufficient number of officers and rank-and-filers, even though he set up Officer Training Corps and supported a “nation in arms” that extended to training young boys as cadets. In spite of these and other deficiencies, however, Spiers contends that Haldane brought as much reform as was politically feasible. By paring the estimates, Haldane gained the support of his cabinet colleagues. Moreover, Arthur James Balfour, having seen his own Unionist party fail to restructure the military, gave Haldane every opportunity to introduce reforms.

This well-researched and closely argued study is a must for anyone interested in British army reform and politics. Most previous studies have depended far too heavily on Haldane’s postwar writings, which were simply the polemic defenses of a sensitive man attempting to rehabilitate his image.

LOWELL J. SATRE  
Youngstown State University

JERRY WHITE. *Rothschild Buildings: Life in an East End Tenement Block, 1887–1920*. (History Workshop Series.) Boston: Routledge and Kegan Paul. 1980. Pp. xviii, 301. Cloth \$30.00, paper \$15.00.

Comparatively luxurious with individual sculleries and w.c.’s, the Rothschild Buildings, built six storeys high in 1887, rose over that part of Whitechapel populated heavily by immigrant Jews escaping from persecution in Eastern Europe. “It was more or less of a little ghetto,” remarked one resident about the buildings that served as a neighborhood and transit point for Jews intent on making new lives for themselves. Model dwellings for the industrious poor were built widely over East London after 1875; 5 percent philanthropy it was called. The Rothschild Buildings, housing 1,000 people in 200 cramped flats, were built for and inhabited by a Jewish artisan class. In 1888, 91 percent of its workers were skilled laborers. It is these people, their lives and labors, who are the concern of Jerry White’s interesting book, a volume in the History Workshop Series.

Daily routines of work, education, and domestic life are clearly delineated in vibrant detail as White paints skillfully, from oral recollections and contemporary records, a pen portrait of people struggling to live. His best chapter reveals the interaction of people in the buildings and within the confines of a few Whitechapel blocks where the immigrants displaced and then lived among the casual English poor. White explores the operation of a Jewish “classic” community wherein existed the physical and psychological helping networks of neighbors and kin. The buildings’ architecture encouraged neighborhoods centering on groups of flats which, in turn, created a melting pot of national Jewish communities soon distinguishable only by their cooking habits. Yet, within a generation all national differences had dissolved, and Yiddish was no longer the common tongue. The children of immigrant Jews had largely become Anglicized and moved to the suburbs. White appears to regret this.

This is a story about Anglo-Jewry hinted at by V. D. Lipman and only sketched generally by Lloyd Gartner over twenty years ago. White’s work informatively expands a part of Chaim Bermant’s study of immigration (*London’s East End: Point of Arrival* [1975]), which he does not cite. Without intention, he confirms that many patterns of Jewish working-class life were little different from those of indigenous working men and their families as revealed by Standish Meacham in *A Life Apart* (1977).

Nonetheless, caution is advised. White relies heavily on the memories of twenty-six second generation residents—six of whom are male, and three-quarters of whom were born after 1900—whose average length of tenancy was twenty-six years. Their memories can only address the later years of his study. Chronology is difficult to establish. Appropriate questions are not always asked. What about death? People continually move into flats. Why did they move out? Conclusions of other historians are distorted: for example, Enid Gaudie and Meacham on the repressiveness of dwelling rules (p. 56) and Lipman on apprenticeships (p. 204). No evidence exists that J. N. Tarn or Anthony Wohl has been read, or White would have noted the connection of the Public Health and Cross Acts of 1875 in stimulating model dwelling building.

With such cautions in mind, one may read profitably and pleasurably this fine addition to Anglo-Jewish history.

MICHAEL J. MOORE  
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D. A. FARNIE. *The Manchester Ship Canal and the Rise of the Port of Manchester, 1894–1975*. Manchester: Manchester University Press. 1980. Pp. xi, 187. £44.50.



D. A. Farnie has made another contribution to the local history of Manchester and its hinterland in this study of the ship canal, the port of Manchester that it developed, and the impact of port activities on greater Manchester and adjacent areas since the late nineteenth century. This short book is mainly a narrative, in separate chapters, of the relationship of the canal company and the corporation of Manchester, the development of the Manchester Liners, the import and export trades of the port of Manchester, the industrial estate at Trafford Park, and the impact of the oil trade on the western end of the canal.

Farnie documents that the canal had considerable impact on the economic structure of Lancashire, but nonetheless the general feeling that runs through the narrative is the disappointment that is a common theme in much of the economic history of the north of Britain in the twentieth century. The canal failed to attract the business that had been projected for it, and its finances were precarious at times. The same was true of associated activities.

This study, although it fills a gap in the available history of the region, leaves the reader somewhat disappointed. Often the narrative is presented as a heroic struggle of the canal against the hostility of Liverpool or the established ocean liner companies. This literary device is no substitute for analysis. For example, the relative failure of the canal to attract traffic was the result either of its failure to provide a cost advantage to shippers or of monopolistic power held by the shipping conferences. Farnie hints at the latter, but detailed evidence is never presented. Analysis is needed. Along similar lines, one obvious question is how the benefits of the canal compared to its costs. The text frequently alludes to the canal causing lower freights between Manchester and Liverpool. This is one potentially important benefit of the canal but is never quantified. To be sure, an ex post facto cost-benefit analysis is not all that economic history should include, but surely this is an obvious question to raise about a project like the ship canal.

Farnie has presented a useful work of local history. Unfortunately he has not carried out some fairly obvious analysis that would have greatly enhanced its value.

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ROSS M. MARTIN. *TUC: The Growth of a Pressure Group, 1868-1976*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xiii, 394. \$42.00.

An organization with very modest aspirations at the time of its formation in 1868, the British Trades Union Congress developed in the present century

into a comprehensive and influential trade union confederation, recognized by both governments and employers as the authoritative spokesman for the British trade union movement as a whole. The standard history of the T.U.C.—that by B. C. Roberts—only covers the period to 1921, a date at which the organization had scarcely begun to assume its modern form. Ross M. Martin's study, which covers the whole period down to 1976, is therefore particularly welcome. His book is not, however, as he himself emphasizes, a straightforward history of the T.U.C. in all its aspects. It is a study of the T.U.C. regarded as a political pressure group and is therefore concerned primarily with the organization's efforts to influence the decisions of national government. Since, for most of its history, this was in fact the main function of the organization, Martin's study deals with the key elements involved in the T.U.C.'s rise to its present position in the labor movement.

In his opening chapter the author establishes a framework for the analysis of the T.U.C.'s pressure group activity. He rightly points out that the organization's role is essentially that of an intermediary, standing between government on the one hand and its own affiliated trade unions on the other. Effectiveness as a pressure group depends, therefore, upon the recognition of its authority both by government and its own constituents. These two dimensions of authority—external and internal respectively—are of course interdependent. Thus, for example, the T.U.C.'s internal authority over its own constituents would be enhanced by a situation in which the government accepted its claims to be heard as spokesman for labor. More problematic than this, however, is the question of how much authority the T.U.C. has possessed at any particular time—how effective it has been as a pressure group. In measuring this, Martin concentrates on access to government rather than actual policy achievements.

The account of the T.U.C.'s history that follows, confined as it is within a fairly rigid framework, is somewhat mechanical and even repetitive. The main stages in the organization's development do, however, emerge clearly. Of particular interest is Martin's treatment of the years roughly from 1910 to 1920, when the internal and external authority of the T.U.C. was very weak. The T.U.C.'s path to its present pre-eminent position in the trade union world was clearly not so smooth as has sometimes been assumed. Martin believes that the critical stage, in which the T.U.C. moved into its present authoritative position, occurred in the years 1938-40. From 1940 onwards, he argues, the position of the T.U.C. as a political pressure group remained unchanged in all essentials; these years served simply to confirm and strengthen the authority it had



gained at the outset of the war. To a large extent this may be true, but it hardly justifies the author's device of compressing the entire period from 1940 to 1976 into a single chapter.

JOHN LOVELL  
University of Kent

BRIAN BOND. *British Military Policy between the Two World Wars*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xv, 419. \$48.00.

The British army suffered a lamentable fate in the years between the two world wars. It was treated as the "Cinderella of the Services" and denied the funds offered to the Royal Navy and the Royal Air Force. In addition, British leaders were unable to decide upon the role of the army. They did not know if it should be employed to defend the empire or if it could be trained and prepared to serve as an expeditionary force in France, the so-called "Continental commitment." Decades of neglect in peacetime resulted in the series of defeats that befell the army in the dreadful period between 1939 and 1942. The mistakes in policy of the interwar years are the subject of Brian Bond's excellent history, which is based on a great deal of unpublished material preserved in the Public Record Office and upon other archival sources.

When the First World War ended, a "naïve pacifism" was preached in British schools, churches, and universities. The army estimates were reduced regularly until 1932. In response, the professional officers in the army withdrew into a self-conscious isolation from the rest of British society. They resurrected a pre-1914 style of life for themselves and for the troops that served under them. Although some brilliant leaders emerged as pioneers in the employment of tanks and armored forces, much of their effort to modernize the army was frustrated by conservative superiors. The Germans soon overtook the British in the employment and exploitation of armored divisions, a development that helped to determine the fate of the world. It was not merely military conservatism that produced this awful result. The civilian politicians were so committed to retrenchment and financial stringency that the army's leaders lacked sufficient funds to mechanize their forces.

Neville Chamberlain took the lead in choking the vitality out of the British army. He emerges, in Bond's pages, as the most misguided of leaders. Chamberlain wanted to reduce the army's size and to restrict its functions. He believed that the army's role was to defend the empire as a kind of imperial police force and that Britain's best defence lay in a home-based air force that could serve as a deterrent to German ambition. Bond's book is a notable con-

tribution to the literature concerned with these terrible developments.

ALFRED GOLLIN  
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R. J. OVERY. *The Air War, 1939-1945*. London: Europa. 1980. Pp. xii, 263. £12.50.

In this first general history of the air war of 1939-1945 written in English, R. J. Overy examines the whole war period from the point of view of each of the warring powers and gives us not only a study of military campaigns but also a highly successful examination of aerial doctrine, economic and scientific mobilization, and the political, diplomatic, and military aspects of the management of hostilities. This fine study analyzes the achievements and the failures of the aerial component of the war, placing it in perspective by explaining the role that aviation played in the overall conflict.

According to Overy, the victory of the Allies over the Axis powers in World War II was a triumph of modernity over tradition. Although air power itself did not determine the war's final outcome, its use illustrated dramatically the complexities of managing modern war. Aircraft did not replace land or sea forces during World War II; as in earlier wars, success depended upon the movement of armies to occupy land and the movement of ships to provide supplies and personnel. Germany's initial victories in 1939-41 and the Soviet Union's successes from 1942 to 1945 were due to the maneuvering of great land forces. Airplanes were decisive, however, in several of the great sea battles; notably, no naval guns were fired in the Battle of the Coral Sea, although both sides sustained great losses from air attacks.

For Germany, a strategy of limited air power meant support of the army; for Japan, support of the navy. The Allies' air doctrine, however, involved all four phases of air power: air defense, strategic bombardment, air-naval cooperation, and air support for ground troops. From the beginning, the United States and Britain committed massive resources to aerial warfare.

The Battle of Britain and the Japanese attack on Pearl Harbor fueled the popular imagination regarding the power of aerial operations. The subsequent British decision to target German residential areas and later the American plan to devastate Japanese cities brought the violence of aerial war to civilian populations. In Germany the air war killed more civilians than all British and American casualties combined. Japan lost more lives in the aerial fire bombing of its cities in 1945 than the United States lost in the entire war.

Many scholars have discounted the effectiveness of strategic bombardment; Overy, however, argues convincingly that, although aerial bombardment failed to stem the increase in enemy arms production, it prevented the increase from being considerably greater than it was. In Japan, strategic bombardment, finally involving nuclear attacks, provided a knockout blow, ending the war far earlier than land operations would have.

This good analysis of many studies done on this subject (including the author's own) makes possible a new, balanced synthesis from an objective point of view.

SAM H. FRANK  
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Alexandria

MAX BELOFF and GILLIAN PEELE. *The Government of the United Kingdom: Political Authority in a Changing Society*. (Comparative Modern Governments.) New York: W. W. Norton. 1980. Pp. 438. \$17.95.

In the quarter century after 1945 it was a fairly easy thing to write a textbook about modern British government and politics. So much of the picture seemed to be firmly set: the commanding position of England in the United Kingdom, the monopolistic position of the two major parties, the fixity of party allegiances, the persistence of class as basic to citizens' political identifications, freedom from internal violence and crime, and agreement on full employment and Keynesianism. A politics still rooted in the late Industrial Revolution had not really been challenged.

As this new text by Max Beloff and Gillian Peele demonstrates, developments in the past decade have undercut the old rules of the game without leading to the advent of a wholly new political system. Unprecedented consumer affluence has been accompanied by the resurgence of heavy unemployment and the abandonment of Keynesian methods of countering it. British society has become multi-racial and by traditional British standards unsafe from violence and crime. The major parties have felt the pressures of competitive movements in Scotland, Wales, and Northern Ireland, and voters have reached almost an American level of independent-mindedness. Referendums have been held for the first time in British history. Even contentment with a largely unwritten constitution can be assumed no longer. Agreements reached in Brussels and Strasbourg threaten the sovereignty of Parliament. The task of bringing together an accurate and balanced description of the political state of Britain today is now especially daunting.

Beloff and Peele have succeeded in considerable measure. Their emphasis is more on governmental and political institutions than on the cultural and social background to politics, and their coverage of many key policy issues (such as economic manage-

ment and the structure of the welfare state) is thin. The role of interest groups in collectivist politics is treated cursorily, while the issues derived from liberalism (such as citizen grievances against government actions) are given ample consideration. Although this text does not draw extensively on either historical or social scientific findings, it does provide a reasonable, balanced, occasionally astute introduction to the government of a country beset by problems of interest to students of all postindustrial states.

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PHILIP M. WILLIAMS. *Hugh Gaitskell: A Political Biography*. London: Jonathan Cape; distributed by Barnes and Noble, Totowa, N.J. 1979. Pp. xx, 1,007. \$45.00.

When Hugh Gaitskell died unexpectedly in 1963 at the early age of fifty-six, his stature was so commanding that it seemed doubtful whether the Labour party, which he had led for only seven years, would recover electorally. Eighteen years and three Labour leaders later, that reputation seems less imposing: a promising career aborted, but without any permanent legacy to the party he struggled so valiantly to unify and modernize. Philip M. Williams's monumental official biography does much to refurbish the image of "the most influential exponent of the democratic Socialist ethos" (p. xiii) to show why the sense of loss for a generation of progressive-minded Englishmen has not diminished.

Although Gaitskell's life ended when he was almost on the threshold of 10 Downing Street, he did not at first seem destined for political success. In a meteoric career that brought him the chancellorship of the Exchequer within five years of entering Parliament and the leadership five years later, it was luck and the mistakes of his rivals that explained his victories as much as his own rather pedestrian talents. Aneurin Bevan, the charismatic, left-wing firebrand, forfeited his own chance at the succession by his petulant outbursts and ill-concealed contempt for fellow Labour MPs. Williams regards the Bevanite dissidents as motivated more by animosity toward Gaitskell than by an alternative ideology. After blundering into a futile attempt to expel Bevan from the party, Gaitskell moderated his stance, becoming adept at conciliating many of his former left-wing antagonists, notably Richard Crossman and Harold Wilson, neither of whom repaid his trust with loyalty. It was this post-Bevanite unity that he sacrificed after 1959 in a misconceived attempt to revise clause 4 of the party constitution, an affront to erstwhile trade union allies as well as to party militants. The subsequent squabble over unilateral disarmament and neutralism, exploited,

in Williams's view, by Gaitskell's enemies in an effort to dislodge him from the leadership, revealed his courage and his inflexibility, traits that earned him both respect and opprobrium. The fact that he won that battle, gaining the confidence of Labour's rank and file through reasoned argument, leads the author to surmise that despite some ineffectiveness in opposition, he "might have been the great peace-time leader that twentieth-century Britain has badly needed" (p. 787).

A fair-minded advocate, Williams is determined to uncover every scrap of evidence that sheds light on Gaitskell's public life. Having been only slightly acquainted with his subject, he interviewed several hundred of Gaitskell's friends and colleagues, including some of his sharpest critics. In addition Williams makes extensive use of Crossman's backbench diary, as enlightening about Labour personalities as about its author's discreditable machinations. The leadership struggles and the disputes over clause 4, defense, and the Common Market are so exhaustively examined that this will long remain the definitive assessment of Labour politics between 1951 and 1963. Regrettably, Williams devotes disproportionate attention in this very long book to these later years, with scant consideration of Gaitskell's background and the shaping of his socialism.

In this avowedly political biography, deliberately avoiding personal matters as "inappropriate," the author is invariably judicious. While lauding Gaitskell's integrity and consistency, his aversion to demagogic fervor, he does not ignore his defects as a politician: obstinacy, faulty tactical judgment, and insensitivity to the emotional commitment of others. When dealing with left-wing opponents, however, Williams is less objective, denouncing them as self-seeking opportunists sacrificing party unity to personal vendettas, as "elitist sectarians who posed as the sole guardians of Socialist principle" (p. 261). He reserves his most vehement criticism for Michael Foot, not only a leading Bevanite "irreconcilable," but also the author of an impassioned, polemical biography of his mentor whose allegations Williams refutes in Gaitskell's defense. It is, ironically, Foot who now finds himself cast in the role of his former enemy, desperately striving to unite an even more bitterly divided Labour party.

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JOSEPH M. CURRAN. *The Birth of the Irish Free State, 1921-1923*. University: University of Alabama Press. 1980. Pp. vi, 356. \$22.75.

Joseph M. Curran has written a political history of the formation of the southern Irish state from the last phase of the war for independence to the end of the civil war. Although the story he tells is not new, his assessment of issues and personalities is fresh and

unclouded by partisanship. Although the backbone of the book is political decision making, he never loses sight of the social context in which decisions were made. His clear realization that public opinion was bitterly alienated from the Republican opponents of the Free State sets his work apart from the only other comparably detailed treatment of the same events, Dorothy Macardle's shrill apologetic for De Valera entitled *The Irish Republic*. Although he more often finds fault with Republicans than with Free Staters, he subjects the actions of the latter to searching scrutiny and often sharp criticism. He brings mature judgment to bear upon the moral and political dilemmas of both sides in southern Ireland as well as of the British.

In light of the author's sound insights and new perspectives upon southern Irish and British politicians, it is ironic that when Northern Ireland intrudes into his narrative he lapses into conventional judgments and uncharacteristically superficial analysis. For example, Curran blames Northern Ireland for the fact that the "Council of Ireland"—a joint North-South body contemplated in the 1920 Government of Ireland Act—was never implemented (p. 263). Surely it would be closer to the truth to say that neither of the new states erected in 1921-22 was especially eager to establish the council. For Northern Ireland the council portended Irish unity; for the Free State it would have compromised sovereign independence still further. In 1925 both states seized the opportunity presented by the Boundary Commission fiasco to kill it. In the 1970s, when the Council of Ireland proposal was revived, southern Irish sovereignty was secure, and it was easy to forget, as Curran has, that in the 1920s the Free State hardly lifted a finger to save the council.

Such occasional flaws notwithstanding, *The Birth of the Irish Free State* is an outstanding contribution that is based on very thorough research. Curran's sources include interviews he conducted in the 1950s as well as documents that only became available in the 1970s. Although there may be a limited amount of relevant material in the Irish State Paper Office that he has still not been able to consult, it is unlikely to alter significantly the picture he has drawn. It would be premature to pronounce this work definitive, but it certainly seems likely to become the standard account of these events for a number of years to come.

DAVID W. MILLER  
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NANNERL O. KEOHANE. *Philosophy and the State in France: The Renaissance to the Enlightenment*. Princeton: Princeton University Press. 1980. Pp. xii, 501. Cloth \$30.00, paper \$12.50.

French political and social thought of the early modern era has been a rich field mined by scholars

of substantial intellect over the past two decades. Such scholars as William Church, Donald Kelley, Lionel Rothkrug, Peter Gay, A. O. Hirschman, and others have revealed an intricate world concerned with the classical issues of politics—state, sovereignty, legitimacy, power—yet demonstrably different from the Anglo-American traditions that shape so much of our thinking. Nannerl O. Keohane now offers a knowledgeable, lucid venture toward synthesis, drawing both from the scholarship of others and clear mastery of the original sources.

Keohane guides the reader through discussions of the now familiar sequence of French political theorists from Seyssel to Rousseau. The discussions center on three types of thought: constitutionalist, absolutist, and individualist. The first two categories will not surprise scholars. Discussions of such figures as the absolutist Bodin and the constitutionalist Hotman must, perforce, traverse much familiar territory. It should be noted, though, that even in these familiar areas Keohane brings together the best recent scholarship with new and nuanced readings.

More provocative is the role assigned to individualism in the history of French political thought. Individualism is clearly not as well defined as the other two categories. Indeed, it seems to function more as a leaven for the formulas about state and sovereignty generated by formal political thinkers. In such diverse thinkers as Montaigne, Pascal, and Montesquieu, Keohane shows at work a persistent concern for the relationship of individual character to public order. Hence she shows how such categories as *amour-propre*, *amour de soi*, and *honnêteté* could be loaded with political as well as moral implications. She rightly argues that this persistent concern with the psychology and character of the individual was fruitfully subversive of the classical dichotomy posed by constitutionalist and absolutist theorists. Rousseau stands as the paradoxical culmination of this subtle interplay: a radical democrat, intense moralist, and notorious individualist, who nonetheless defends a sovereignty seemingly as unbridled as that of the most ardent absolutist.

No doubt scholars will disagree with certain individual readings and speculations. Is it true, for instance, as Keohane argues, that Rousseau's legislator is a clear analogue to the absolutist's sovereign? For Rousseau the legislator was a *deus ex machina*, designed like Moses and Lycurgus to set things right and then disappear; such cannot be said for the sovereign of the absolutists. Despite the book's length, one might have wished for more context: a deeper analysis of the interplay of political thought with political event and a more sustained comparison of France to the parallel yet different evolutions in England and Eastern Europe.

Still, the final judgment must be that this is a work of great utility. For someone just venturing into the field, it provides a lucid structure to be tested against further reading. For the scholar of French social thought, it offers a challenging reading of a sequence of theorists who have been amply explored as individuals but never brought together before with such striking effect.

HARRY C. PAYNE  
Colgate University

HUGUES NEVEUX. *Vie et déclin d'une structure économique: Les grains du Cambrésis (fin du XIV<sup>e</sup>-début du XVII<sup>e</sup> siècle)*. Foreword by EMMANUEL LE ROY LADURIE. (Civilitisations et Sociétés, number 64.) New York: Mouton Éditeur or École des Hautes Études en Sciences Sociales, Paris. 1980. Pp. xiii, 443.

Specialists in French rural history are already familiar with Hugues Neveux's research on grain production in the Cambrésis from summary articles. Now they have the integral, unrevised text of his 1973 thesis. This is both a major contribution to the field and a work of formidable complexity.

The Cambrésis was a small ecclesiastical county on the present Franco-Belgian border. This hapless region was devastated no less than five times during the military campaigns of the Hundred Years' War, the troubles accompanying the disintegration of the Burgundian state, the Habsburg-Valois wars, the revolt of the Netherlands, and finally the Thirty Years' War. Economically, the Cambrésis was part of a larger region of sheep and grain farming that included Picardy, Artois, and Hainaut. Producing wheat, wool, and mutton for local markets and for export to the population centers of the southern Netherlands, the Cambrésis enjoyed an advanced agricultural economy that rested essentially on medium-sized farms of 20 to 50 hectares. These belonged primarily to ecclesiastical landlords and were worked by well-to-do tenant farmers who exercised dynastic control over leases and rentals from as early as the fourteenth and fifteenth centuries.

Neveux found a very high level of wheat and oats production for the early, pre-Black Death fourteenth century (index 140 for wheat) and a major drop between 1320-40 and 1370 (index 100). Thereafter, wheat production fluctuated in the range of 10-20 percent, major crises apart, and stabilized in the sixteenth and seventeenth centuries (index 80-95) at a level well below the early fourteenth-century peak. Indeed, these early levels were not achieved again or surpassed until the late eighteenth and nineteenth centuries when substantially different farming techniques spread. Neveux argued that the high levels of production for 1320-40 were the result of fairly intensive mixed



farming practices: large flocks of sheep; peas, beans, and lentils sown with oats on the second field and perhaps also grown on part of the fallow. This economic structure collapsed slowly in the late Middle Ages under the blows of population loss and declining markets for wool as the region shifted from woolen textiles to linen. The cultivation of legumes tapered off, flocks thinned, grain yields fell. Wheat production, although down, held up better than oats, which fell to half the 1320-40 level by the beginning of the seventeenth century. Finally in the sixteenth century, Baltic grain imports in the Netherlands made wheat from the Cambrésis less attractive.

Neveux was particularly interested in the responses of tenant farmers to market conditions in the sixteenth century. The export market for grains weakened and demand for wool dampened, but the local market for grains expanded with the growth of population in the Cambrésis. Here was an opportunity for increased production, but the tenant farmers did not respond. Why? Neveux's answer was something of a surprise since he followed a straightforward economic analysis to this point. He argued that although tenant farmers knew the methods of more intensive farming, they did not apply them. Rather, they preferred the easy profits of price inflation for grains and the social benefits of consolidating their power in the peasant villages that dynastic control of the large farms ensured them.

As is to be expected, the data are both fragmentary and extremely difficult to understand. Although it makes for laborious reading, it is to the author's credit that he always spelled out exactly where the hard data ended and the speculation began. Now Neveux may well be correct in his reading of the evidence, but alternatives are perhaps also plausible.

The data for the unusually high levels of production for the early fourteenth century are very suspicious and scanty. Remove or scale down these figures and you are left with four centuries of stability with minor fluctuations in wheat production, periodic crises apart. The virtual immobility of rentals from the early fourteenth to the early seventeenth centuries for the most prized farmlands also suggests stability of economic structure. As for sheep farming and markets for wool, few topics are harder to pin down. Sheep were not subject to the tithe in this region, and the price data on wool and animals ends in the middle of the fifteenth century. A. Verhulst, for one, is of the opinion that the Cambrésis continued to be a major producer of wool throughout the entire period under discussion.

The above is not intended as a devastating criticism of Neveux's book. Scholars are in substantial disagreement on virtually every issue raised and the

alternative interpretation presented here is only one of several possible ones. Neveux's meticulous research is and undoubtedly will remain a major contribution, whatever the final verdict may be on the finer points of interpretation.

JAMES L. GOLDSMITH  
University of Oklahoma

MICHEL VOVELLE. *Ville et campagne au 18<sup>e</sup> siècle: Chartres et la Beauce*. Foreword by ERNEST LABROUSSE. (Problèmes-Histoire.) Paris: Éditions Sociales. 1980. Pp. 307.

The Beauce, whose eighteenth-century boundaries were nearly coterminous with those of the present department of Eure-et-Loir, and Chartres, the area's major city, are the subjects of *Ville et campagne au 18<sup>e</sup> siècle* by Michel Vovelle. A member of the faculty of the Centre méridional d'histoire sociale des mentalités et des cultures at the University of Provence (Aix), Vovelle has become a leading authority on the *ancien régime* and Revolution in the Midi and, more particularly, in Provence.

*Ville et campagne au 18<sup>e</sup> siècle*, which focuses on an area just to the southwest of Paris instead of on the south, seems to be a departure for Vovelle. The explanation is that this book was originally written about twenty years ago under the direction of Ernest Labrousse, who has contributed an introduction to the current volume. Each of the eight chapters was published in an earlier version between 1958 and 1963; five of them were read at the Congrès national des sociétés savantes (Bordeaux, 1957; Aix-Marseille, 1958; Dijon, 1959; Chambéry-Annecy, 1960; Montpellier, 1961) prior to their initial publication.

The book is divided into three parts. The urban-rural tension in the Beauce is examined in the context of Chartres, the regional capital, in the first section. The second is concerned with those social groups, especially the bourgeoisie and the cathedral chapter in Chartres, that were the beneficiaries of *rentes*. The cathedral chapter was by far the largest landed property holder, owning nearly 7,000 hectares in the Beauce, 124 seigneuries, 113 houses in the city itself, 20 mills, and at least 30 large storehouses where its dues paid in kind were collected and stored. Because of its wealth and the large number of nobles who belonged to it, the cathedral chapter was envied and despised by virtually all classes of society with which it came in contact, including the parish clergy who hardly concealed their contempt. The loss of its income—and the property associated with it—would have a tremendous impact in the area, affecting the peasants as well as the bourgeoisie.



The third part deals primarily, but not exclusively, with the Revolution as a reflection of tensions that already existed in society. Here Vovelle emphasizes the pernicious effects of the twin evils of high grain prices and the scarcity of bread. It is in this context that Vovelle examines the popular uprisings that occurred between the Seine and the Loire during the course of 1792. He presents an excellent analysis of those rural areas, with their forests, secrets, and hidden grievances, and their growing resentment of the *rentiers* and of profiteers in general.

Vovelle's ability to draw detailed comparisons between the Beauce and the Midi is invaluable, as is his discussion of the variety of sources available in departmental archives. We have come to expect from Michel Vovelle excellent scholarship, insight, perspective, and a distinct feel for the nuances present in society, all of which are abundantly evident in the present volume.

THOMAS F. SHEPPARD  
College of William and Mary

JOHN L. SUTTON. *The King's Honor and the King's Cardinal: The War of the Polish Succession*. Lexington: University Press of Kentucky. 1980. Pp. vi, 250. \$19.50.

For Poland, it was an ominous sign of things to come, but from a European perspective the War of Polish Succession was primarily a chapter in the continuing struggle between the Bourbon and Habsburg dynasties. The war was fought, for the most part, along the Rhine and in northern Italy. After three years of desultory fighting, a peace was made that recognized the Russian-Austrian candidate, Augustus III, as king of Poland and rewarded France's allies, Spain and Sardinia, with Italian territories. More to the point, the duchy of Lorraine was awarded to the defeated French candidate, Stanislas Leszczynski, father-in-law of Louis XV. This assured the eventual possession of Lorraine to France. Because the French energetically pushed Leszczynski's claims and then denied him adequate military support, the question of French intentions has puzzled historians.

John L. Sutton, using the archives of London, Vienna, and Paris, has written of the Bourbon-Habsburg conflict, primarily from a French perspective. His view of the conflict rests on his contention that the War of Polish Succession epitomizes the classic diplomacy and limited wars of the eighteenth century. His explanation of French behavior is in keeping with this view: Cardinal Fleury was opposed to a full-scale effort on Leszczynski's behalf lest it antagonize the British. Louis XV's honor required, however, that an effort be made.

Therefore, the war was fought where it would not provoke English intervention—against the Austrians on the Rhine and in Italy. In this view, French aims were not the throne of Poland but to gain some sort of satisfaction for Leszczynski and sustain the king's honor.

Although overly tentative in his presentation, Sutton sheds some interesting light on the motives and conduct of the war. He points out that the war was conducted by aging leaders whose reputations had been earned in the wars of Louis XIV. There is also a brief but fascinating discussion of atrocities and the problem of morale and discipline. It is in remarks such as these that the book's greatest value lies.

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JAMES F. TRAER. *Marriage and the Family in Eighteenth-Century France*. Ithaca: Cornell University Press. 1980. Pp. 208. \$15.00.

In this essay in intellectual and legal history, James F. Traer attempts to show how the ideas of the Enlightenment and the French Revolution of 1789 changed concepts of marriage and the family in France and prepared the way for the rise of the "modern family." Traer follows in the footsteps of Edward Shorter and Lawrence Stone and defines the modern family as one based on sentiment, affection, and free choice rather than on obligation or material considerations. Traer argues that it was not the economic or social changes of the late eighteenth and early nineteenth centuries that were responsible for the creation of the modern family but rather "certain kinds of institutional conflicts, such as a dispute between the Catholic Church and the French Monarchy, [which] yielded a new view of marriage and the family" (p. 18). This vague explanation is suggestive of much that is problematic in this work.

Traer begins by showing how royal authority under the *ancien régime* gradually eroded the jurisdiction of the church by emphasizing marriage as a contract and by minimizing the importance of marriage as a sacrament. The Enlightenment *philosophes* contributed to this change by stressing the secularization of marriage and toleration of the marriage customs of non-Catholics and by emphasizing sentiment and affection in conjugal relations. In addition, numerous Enlightenment writers translated their belief in liberty and equality for individuals into arguments for the right of individuals to divorce.

The growing effort of the state to define the terms of marriage and divorce through civil law resulted in several major reforms, making it easier for both

men and women to obtain divorces, changing inheritance laws to provide for the equal distribution of patrimony to legitimate heirs, and introducing a family court (*tribunal de famille*) that was designed to introduce a measure of equality in family relations by removing some of the authority of the father in settling family disputes.

Despite the revolutionaries' efforts to establish equality and liberty as the guiding principles governing the relations between men and women in marriage and divorce, much of that work was pushed aside by the Napoleonic Code. Although it retained some of the revolutionaries' ideas, notably on inheritance and divorce, the Code re-established the authority of the father as supreme within the family and left no doubt about the inequality of husbands and wives.

Traer's very competent examination of the legal changes made by the Revolution leaves much to be desired from the standpoint of the social historian. His work raises an old question, but one that evidently still needs asking: how much can be concluded about real life from prescriptions for social behavior (such as those that the law attempts to provide)? Traer argues, for example, that the creation of the *tribunal de famille* demonstrates that the revolutionaries were trying to introduce sentiment into the resolution of family disputes by making the arbiters of disputes family relations rather than unknown lawyers or judges. Yet, despite the intentions of the lawmakers, families seemed reluctant to accept this system and increasingly brought in outside arbiters (lawyers and judges) rather than rely on the not always impartial arbitration of relatives.

A more fundamental problem, however, concerns the background and explanation of the revolutionary legislation. Traer ignores the fact that the revolutionaries' new attitudes toward the family really reflected new perceptions of the entire economic and social order and thus went much deeper than the need to legislate on a particular issue. His insistence that the changing attitude toward marriage and the family was the product of a legal revolution nurtured by ideological change alone leaves out the essential socioeconomic transformation of French society to which ideology was intimately connected. Thus his account of the attention given by the state to inheritance in the interest of facilitating family management of property belies his contention that economic and class interests were unimportant (p. 184).

Finally, what does the apparent abandonment of the revolutionary ideas of liberty and equality by the Napoleonic Code suggest about the view of the family that finally prevailed (and some of which is still reflected in contemporary French law)? Why was that revolutionary legislation so ephemeral? Clearly, the ideals of liberty and equality, romantic

love, and domesticity did not so easily or so quickly succeed in creating, even in legal terms, the new family that Traer wishes to see emerging triumphantly at the start of the nineteenth century.

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CARMINELLA BIONDI. "*Ces esclaves sont des hommes*": *Lotta abolizionista e letteratura negrofila nella Francia del Settecento*. Foreword by CORRADO ROSSO. (Studi e Testi, number 55.) Pisa: Editrice Libreria Goliardica. 1979. Pp. ix, 314.

In "*Ces esclaves sont des hommes*," Carminella Biondi traces the rise of abolitionist thought in France in the eighteenth century through the first decree of abolition in 1794. She examines the effects of such a struggle first on French enlightened and revolutionary thought and second, in a real sense, on the colonies directly affected. Biondi believes and endeavors to prove that France was in a special position in Europe to bring about such a major alteration in human relationships. She argues that abolitionism and the political trend toward revolution did not necessarily follow the same progression, but once the ideals of the Enlightenment were loosed in revolution, abolitionism—a natural outgrowth of those ideals—had to follow. Although work by men like Abbé Henri Grégoire culminated in the 1794 decree by the National Convention, the decree, according to Biondi, was not simply a natural result of 1789.

Biondi argues that the problems of slavery and the relationship of Europeans with the natives of the newly discovered areas in Africa and the Americas were recognized early. The writings of Las Casas and others were translated into French and had a definite impact on eighteenth-century philosophers such as Rousseau and Montesquieu. Accounts of early explorers, ascribing to these newly discovered areas the attributes of a "new creation" filled with "noble, peaceful folk," influenced those who idealized man in a state of nature.

Biondi does indeed back up her basic thesis with convincing evidence, presented with clarity of style. Well footnoted, "*Ces esclaves sont des hommes*" is not a philosophical essay but a scholarly work of the first order. Biondi's bibliography is extensive, including classic works as well as the newest French, Italian, and English scholarship. The reader wishes that a note had been added concerning the availability or nonavailability of archival sources, since no archival sources appear in the bibliography.

Biondi's work fills a void in French history and in the area of the history of the struggle against slavery. Most historians realize the intensity of abolitionist thought during the French revolutionary period, but with Biondi's detailed investigation and

analysis the antislavery idea becomes a very definite thread in the whole fabric of French history during the Enlightenment. Her book, as yet only available in Italian, expands our insights into the ideals of the Enlightenment and of revolutionary France.

JAMES J. COOKE  
University of Mississippi

DARLINE GAY LEVY. *The Ideas and Careers of Simon-Nicolas-Henri Linguet: A Study in Eighteenth-Century French Politics*. Urbana: University of Illinois Press. 1980. Pp. x, 384. \$30.00.

This excellent biography carries our understanding of Simon Linguet and his place in his society much beyond what to my knowledge has previously been published. Darline Gay Levy has utilized previous studies on Linguet and related subjects, but the great merit of her research lies in the quantity and variety of her contemporary sources, including rich manuscript materials in public libraries and archives in France, Belgium, Switzerland, Austria, and England.

Linguet's *Annales*, published abroad, were avidly read and widely distributed, but it is not the business of writing and publishing a journal that chiefly concerns Levy. She is primarily interested in Linguet's social ideas, which she develops at length and in detail, and in his attempts to win a hearing for them at the centers of power and, by such means, to achieve fame and a position of honor for himself.

Levy addresses the question, was Linguet a conservative or a revolutionary, and the answer is not simple. Linguet saw society as divided between the oppressors—the rich and powerful—and the oppressed, including himself. The most oppressed and miserable were the laboring poor, whose lot was progressively worsening, and Linguet forecast an imminent revolution whereby they would destroy existing society. How to prevent this? For Linguet was never really a revolutionary himself. Philosophic liberalism he despised, above all Physiocracy and the tradition of Montesquieu, which he thought only tools of the oppressors. Linguet held that absolute monarchical power must step in to guarantee every subject his property, not only in land and goods, if he had any, but also in life (which involved an adequate food supply and subsistence wage), in his profession, reputation, and the liberty of his person. All this is styled in Levy's exposition, "liberty of dominion." Continually publicizing his own grievances (disbarment from the practice of law, summary dismissal from editorship of the privileged *Journal de politique et de littérature*, and arbitrary imprisonment in the Bastille for nearly two years), Linguet fought tenaciously against Physiocrats, academicians, his colleagues of the bar, the parlements, ministers, and certain indi-

viduals. Strangely enough, he also enjoyed at times a certain patronage in high places, notably with the king and queen of France, Emperor Joseph II, and some officials in the Austrian Netherlands.

The Revolution in France did not basically change Linguet's perspective. Until 1792 he was still urging on the king the idea of a "populist monarchy," and even accepted royal moneys. In the same period he proposed to the National Assembly a plan for a *caisse nationale* that would among other things function on a vast scale as a social security agency, with the object of averting the revolution of the masses that had not yet come but was, he warned again, imminent. To no avail.

His correspondence of 1792 with the king having come to light, Linguet was tried and guillotined in 1794 as "a partisan of despotism." His "real crime against the Revolution," in Levy's conclusion, was to have "tried to program only the material dimensions of the liberty of dominion" and to have failed to appreciate "the reigning revolutionary myths" and "priorities" of an emergent political democracy (p. 333).

FRANCES ACOMB  
Duke University

MARTIN S. STAUM. *Cabanis: Enlightenment and Medical Philosophy in the French Revolution*. Princeton: Princeton University Press. 1980. Pp. xi, 430. \$27.50.

Martin S. Staum's study of Cabanis is a contribution to the growing body of literature on the thought and influence of the *Idéologues*. A major theme of the book is the role of the Enlightenment in the development of the social sciences or, in the common designation of the time, the science of man. The ideas and activities of Pierre Cabanis (1757–1808), the *Idéologue* physician, whose medical philosophy and political connections brought him to prominence in the early years of the Revolution, make him a suitable focus for such a discussion. Cabanis's goal was to establish a science of man that united physiological theories, Condillac's epistemology, and questions of ethics.

Staum's account interweaves discussion of Cabanis's career and ideas with his relationship to other exponents of *Idéology* and seeks to evaluate his lasting contributions to moral philosophy, medical organization, and biological theory. Cabanis's philosophical beliefs were shaped by his association with the scholars in Madame Helvétius's circle. His medical outlook was influenced both by the vitalism of the Montpellier school and by the ideas on the value of clinical experience in medical training that were gaining currency in the seventies and eighties. In the early years of the Revolution, Cabanis became involved in the endeavors to reform

hospitals, education, and public assistance. He weathered the crises of 1792–1795 and, in the latter part of his life, achieved both medical and political eminence. This period saw the publication of his best-known works, *Coup d'oeil sur les révolutions et sur la réforme de la médecine* (1795, pub. 1804) and *Rapports du physique et du moral de l'homme* (1798–1802).

It is Staum's claim that Cabanis's belief in the unity of nature and the unity of the sciences did not stem from a materialist or mechanist point of view. While Cabanis thought that human passions and ideas had a physiological basis, sensitivity remained for him an irreducible property of life. Both human beings and society were perfectible according to Cabanis, but, in moving toward these goals, a compromise had to be sought between freedom and regulation.

Staum's account is useful for the insights it provides into the complex relationships between medicine, politics, and philosophy in the late Enlightenment and during the Revolution. It helps our understanding of the developments in professional reform and physiological theory that characterize Paris medicine of the early nineteenth century. The book is well produced and adds to the studies of Gusdorf on the science of man and Moravia on the *Idéologues* including Cabanis, none of which have appeared in English. It also complements Keith Baker's important book on Condorcet, although it perhaps lacks the substance and subtlety of this work.

CAROLINE HANNAWAY  
Johns Hopkins University

HANSGEORG MOLITOR. *Vom Untertan zum Administré: Studien zur französischen Herrschaft und zum Verhalten der Bevölkerung im Rhein-Mosel-Raum von den Revolutionskriegen bis zum Ende der Napoleonischen Zeit.* (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Universalgeschichte, number 99.) Wiesbaden: Franz Steiner Verlag. 1980. Pp. xiv, 248. DM 64.

Hansgeorg Molitor's essay joins a growing list of relatively recent works that have attempted to illuminate the impact of the period of the French Revolution on the growth of German national consciousness and, more broadly, of German political consciousness in general. Limiting his investigation largely to those areas of the left bank that became the department of the Rhine-Moselle, and to the period of French rule from 1794 to 1814, the author examines the French administration of these territories and the reactions of the German inhabitants to that administration in order to determine whether significant changes in attitude resulted from this period of political transformation.

The author's conclusions, which will probably come as no great surprise to those familiar with the literature, indicate not only that there was no meaningful growth of German nationalist feeling but also that no fundamental alteration in attitudes toward basic questions of public or political life occurred in the area during this period. The Rhine-Moselle region remained essentially both peaceful and obedient to its new masters. In general, the progressive stabilization of German attitudes toward the new regime paralleled the continuing stabilization of the administration itself. Thus, many evidences of discontent apparent during the period of occupation—a time of considerable confusion and arbitrariness on the part of military authorities—disappeared after a major reform of 1798, which really began the administrative assimilation of these left bank territories into the French Republic. Acceptance increased still further with the continuing perfection of the administration under the consulate and empire, lending credence to Molitor's assertion that it was the efficiency of the administration, not primarily the pressures of the so-called Napoleonic "police state," that produced German accommodation to, and even some approval of, the French regime.

Negative attitudes existed, to be sure. But Molitor convincingly demonstrates that their causes neither arose from nor were productive of nationalism or any other ideology: objections to military service, resentment of high taxes and military requisitions, war weariness, and so forth—all of which were sources of popular dissatisfaction under the old regime as well. Even genuine feelings of attachment to their old rulers and constitutions felt by many Rhenish officials were weakened by fear of unemployment and other factors and disappeared almost altogether after 1801 when the Peace of Lunéville finally and formally awarded the left bank to France and eliminated the problem of conflict of loyalties. Thus, although opposition to particular policies of the French administration existed, a *résistance* in the sense of an even loosely organized or systematic opposition did not.

Well researched, partly from both French and German archives, Molitor's work, though limited in scope, is an interesting and solid addition to the literature.

JOHN G. GAGLIARDO  
Boston University

HUGH CLOUT. *Agriculture in France on the Eve of the Railway Age.* Totowa, N.J.: Barnes and Noble or Croom Helm, London. 1980. Pp. 239. \$28.50.

This compact and useful book, originally a doctoral dissertation, was inspired, the author tells us, by the



frustration he experienced as a historical geographer at "the near impossibility of charting rather than inferring social and economic processes in nineteenth-century France." His frustration is surely shared by many students of French agricultural society: although France remained half rural and almost half agricultural into the twentieth century, we know about the development of this world chiefly what incredibly detailed investigations by specialists of comparatively small regions can tell us. Hugh Clout has not presumed the daunting task of charting the general evolution of French rural society through the nineteenth century. More modestly, he has with this book offered what he calls "a datum plane . . . of interest to process-orientated researchers in the future."

What this means essentially is that he has carefully digested the earliest volumes of government-published statistics and with the aid of computer-generated cartography transformed the numbers there reported into an *arrondissement-by-arrondissement* comparative picture of French agriculture midway through the July Monarchy. A reviewer, of course, might assail him for the unreliability of his principal source, for the data ultimately derive from the reports of village mayors, functionaries notoriously prone to error if not deception. But as Clout notes, we have nothing else to go on. Though the *Statistique* is "an estimate . . . not a rigorously precise portrayal of agricultural reality," we must make the best we can of it.

Clout has in fact exercised considerable subtlety to make these suspect figures speak. His "background" chapters—on population, the food supply, and provincial diversity—are disappointingly conventional and in some cases not quite up to date (for example, van de Walle's reconstruction of nineteenth-century population is not cited). The maps in his later chapters, by contrast, bring home vividly to the reader how little regional specialization in land use had progressed in the 1840s and yet how dramatically productivity could vary (a fourfold difference in yield of hectolitres of wheat per hectare, for example, between Brie and Gourdon in the Lot). In perhaps his most original chapter, "Feeding the People," Clout ingeniously converts reported volumes of foodstuffs into weights and from these derives calorific values that reflect a dietary "mosaic" of France as complex as that of agricultural production itself. He thus ironically confirms the very difficulty of generalization that first inspired him: rural France in the 1840s was not one country, but hundreds of them.

Even so striking a map as that of total intake of calories per capita per annum in each *arrondissement* in one sense provides no real surprises for informed readers. Well-nourished France, like productive France, was largely northwestern France, well

above the famous line forever being drawn between Saint-Malo and Geneva. Yet reflective historians will find in this, as in almost every map, food for thought on subjects ranging well beyond that of the book. What are the social and political consequences to be inferred, for example, from the facts that wheat prices in Provence were consistently higher than anywhere else and that caloric intake there was mediocre at best? How could Clout's detailed portrait of a still widespread "economic *ancien régime*" help the demographic historians explain the spread of family limitation in the "hungry forties," as population pressed against levels of subsistence? Did Napoleon III arrive with his railways in the nick of time for the French countryside as his regime seems to have arrived opportunely with modern urbanism for Paris? These questions suggest a verdict on Clout's book that any author should be glad to deserve: it is the kind of work that can stimulate the most suggestive of historical insights, though it does not provide them itself.

JOHN ROTHNEY  
Ohio State University

WILLIAM H. SEWELL, JR. *Work and Revolution in France: The Language of Labor from the Old Regime to 1848*. New York: Cambridge University Press. 1980. Pp. x, 340. Cloth \$35.95, paper \$8.95.

The most resolute and intractable opponents of the "new social history" have been the historical sources. Labor historians have turned to censuses, court reports, and vital statistics records to study the daily lives of the masses. But such quantitative sources shed little light on popular opinions or community sentiment. To remedy this deficiency, William H. Sewell, Jr.'s book suggests fresh ways to study working-class culture and raises new arguments about the importance of plebian ideology.

Drawing on the works of anthropologists such as Clifford Geertz and Victor Turner, Sewell analyzes the culture of French artisans by paying close attention to workers' rhetoric and the subtle shifts of meaning in popular pronouncements. His conclusions are extremely interesting. An examination of the persistence and evolution of terms like "corporation," "corps," and "état" leads Sewell to conclude that pre-revolutionary corporatist perceptions still influenced the vision of nineteenth-century artisans. In fact he traces the intellectual roots of the socialism of 1848 to a merger of conceptions of occupational solidarity and community loyalty inherited from the *ancien régime* with conceptions of popular sovereignty and the rights of labor springing from the Revolution.

A thorough knowledge of French social and economic history informs this study of working-class



consciousness, but its central purpose is to use intellectual history to place artisanal political doctrines into a larger context. Sewell's examination of the fundamental changes in the definition of property wrought by the Revolution of 1789 is the kind of synthetic analysis that reveals new facets in each individual component of the argument. His work convincingly demonstrates that mid-nineteenth-century socialism partially derived from *ancien régime* corporate organizations via mutual aid societies and producers' associations.

Sewell outlines a challenging research agenda for French labor historians. More needs to be known about the boundaries of the culture he describes and the population this culture encompassed. Were there one or many artisanal cultures? More also needs to be known about the behavior and protest strategies employed by *compagnonnages*, mutual aid societies, and producers' associations. Were these organizations stages in the development of workers' consciousness or did they belong to a stable repertoire of artisanal responses to changing economic conditions and government policies? Although Sewell connects the development of artisanal ideology to the growth of workers' organizations and to periods of political freedom when these organizations were able to develop, his overall emphasis is on the cumulative, step-by-step growth of artisanal consciousness. But the history of the labor movement is not only a history of advance but of retreat and starting over; more attention must be paid to the ways in which work organization and political forces interacted with workers' consciousness to dampen and renew the workers' movement. These reservations are not meant to denigrate Sewell's considerable achievement but to suggest that his techniques and interpretations can be employed most effectively within the framework of a detailed and concrete analysis of working-class social structure and the changing political order.

MICHAEL P. HANAGAN  
Vanderbilt University

JOHN G. GALLAHER. *The Students of Paris and the Revolution of 1848*. Carbondale: Southern Illinois University Press. 1980. Pp. xx, 128. \$9.95.

Intrigued by the French students' revolt of 1968, John G. Gallaher set out to study the role of their predecessors in the Revolution of 1848. He consulted relevant dossiers in the Archives Nationales, the archives of the École Polytechnique, the Prefecture of Police, and the *département* of the Seine, newspapers and other contemporary accounts, and a number of secondary sources. The result is a very short and commendably written book of 128 pages, including six maps of Paris and its *quartiers*, organized chronologically into an introduction and four

chapters: "Background for a Revolution"; "The Making of a Rebellion, February 22 and 23"; "The Rebellion becomes a Revolution, February 24"; "The Students and the Provisional Government"; and a brief conclusion.

Gallaher finds that the participation of the students in the Revolution of February was "crucial," even if they tried to remain neutral during the early hours of the Revolution and did not join the insurgents in large numbers. Angered by the abrupt and politically motivated dismissals of their favorite professors, Edgar Quinet, Jules Michelet, and Adam Mickiewicz, and sharing the desire for reform with the liberal bourgeois opposition to Louis Philippe, those students who marched to join the crowd at the Place de la Madeleine on February 22 "were instrumental in directing and sustaining the demonstration that led the Parisian population to rise up against a government it wished to reform" (p. 105). Thereafter they served as keepers of the peace and soon as messengers for the new provisional government. No radicals were they. When one group of *polytechniciens* encountered workers marching with a red flag on February 24, their student sergeant shredded the feared *drapeau rouge*. After the new government reinstated their professors, most of the students, as good bourgeois, opposed the workers in the June Days.

Small, concise books can be even more useful than their more bulky counterparts, but this thin volume could itself have been reduced to an article. This study contributes little that will influence our interpretation of the Revolution of 1848, though it serves to remind us in what ways the *journées* of February and those of June were different. The introduction is sketchy and not terribly well informed. The descriptions of education in Paris and of student life in the 1840s are largely drawn from published secondary sources. Most of the chronologically organized text provides a rather standard, familiar play by play account of the February Days, including the obligatory "When France sneezes all of Europe catches a cold" (p. 88). The conclusion that the students were reformers and not revolutionaries is not surprising, and often repeated, leaving the reader wondering if the topic, here so narrowly defined, merits a book-length study, even a small one. An article format would have better served to accentuate the author's contributions: some discussion of the *comité central des écoles*, the students' papers *L'Avant Garde* and *La Lanterne*, and particularly the chatty accounts of the experiences of individual students during the February Days. The latter include that of one poor fellow who was injured falling from the top of a barricade. Most of his *confrères* did not get that far.

JOHN MERRIMAN  
Yale University

ALBERT BOIME. *Thomas Couture and the Eclectic Vision*. New Haven: Yale University Press. 1980. Pp. xxii, 683. \$75.00.

This substantial book is remarkable on several counts. Albert Boime, using the career and paintings of Thomas Couture as his basis, has written a complex cultural history of mid-nineteenth-century Paris. Yet it is not simply a general cultural investigation, because Boime subjects Couture's work to various treatments that are traditional to the history of art—style, "sources," iconography, and so forth. A rich and exhaustive study, remarkable in the range of its information, it is methodologically as eclectic as Boime maintains Couture's painting to be.

Thomas Couture (1815–79) was by no means a typical mid-nineteenth-century French painter, either in his way of painting or in his effect on others, although he was often considered safely middle-of-the-road. Rebellious but not revolutionary, he was never able to ally himself for long with any political or artistic faction. His life was like his painting, filled with episodes of brilliant promise that were never quite brought to an equally brilliant fulfillment. His great success at the salon in 1847 with *Romans of the Decadence* was never equaled. I am less inclined than Boime to equate Couture's problems with the cultural and political conditions of his time; there were other painters no less involved who emerged quite differently. Although his artistic structure was built on the eclectic principles that underlay much contemporary thought ("eclectic" and "juste milieu" are tiresomely recurrent terms), that which was most influential in Couture's works was the product of his own inquiring but troubled nature. The extensive and fascinating philosophical, literary, and political milieus abundantly provided by this study complement but do not explain the peculiar character of Couture. Yet the adroitly documented cultural fabric woven by Boime is admirable in its own right and serves as a welcome antidote to the simplistic generalities habitually used to bolster a theory of social causality.

It is only just that the major study of Couture should be produced by an American scholar. In his later life Couture largely isolated himself from his compatriots and devoted much of his energies to teaching foreign students, many of whom came from the United States. He became a comforting sage to the aspiring young who were drawn neither to the academy nor to the most daring tendencies in art; his little book, *Méthode et entretiens d'atelier* (translated into English in 1879) had an extraordinary impact and was hailed as an inspiration in the United States by both artists and patrons. Boime has treated at length Couture's relationship with his pupils and unwitting imitators, and if he has

claimed rather too much for his man, his excess can be considered only just retribution.

The haunting question that arises is why a considerable number of those who studied with Couture or took their aesthetic cues from his teaching achieved greater stature in art than their master. For example, as Boime carefully documents, Edouard Manet was his assiduous student for many years. Yet regardless of the parallels one may find between the two men—and Boime points out many—Manet's painting at its best exists in a totally different realm of artistic quality than that of Couture. Although Couture may have revolutionized technique, Manet revolutionized our perception.

But this is not the kind of question Boime addresses, nor could it be treated readily by the descriptive and documentary method he uses. Turning his back on the tiresome assumption that great painters beget great painters in a kind of aesthetic vacuum—an assumption that has done much to obscure the actual history of nineteenth-century art—he concerns himself with the intricate network of relationships with contemporaries and with past art that made up the artist's world. Although Couture does not emerge as a star of rediscovered brilliance in our artistic firmament, Boime's book makes a monumental contribution to our understanding of art and thought in the nineteenth century. One need not agree with Boime's suggestion that our present acceptance of cultural plurality is akin to nineteenth-century eclecticism in order to recognize the value of this detailed and erudite study concentrated on one corner of the modern world.

The book is well designed, copiously illustrated, and supported by extensive notes and bibliography.

JOSHUA C. TAYLOR

National Museum of American Art

STEPHEN A. KIPPUR. *Jules Michelet: A Study of Mind and Sensibility*. Albany: State University of New York Press. 1981. Pp. xiv, 269. Cloth \$34.00, paper \$12.95.

Stephen A. Kippur's *Jules Michelet* recounts the historian's remarkably integrated life and works in a brisk, scholarly manner, yet it is not so much a full biography as it is a study of Michelet's historical and political thought. Much more attention is given to the histories than to the revealing diaries, letters, or other writings, and there is no trace of the provocative psychological exploration that Roland Barthes pioneered in Michelet studies several decades ago. Apart from occasional passages such as the fine psychological reading of Thomas Couture's portrait, the book is short on scrutiny of the "inner

man"—his restlessness and love of travel, his sexual ideas, his sorrows and self-doubt. Nor does the book closely portray his orderly home life—his arduous and methodical work habits, his numerous family responsibilities and concerns. Kippur's industrious archival research, however, has yielded interesting details on such matters as Michelet's administrative work as national archivist, the poor sales of his *Histoire de la Révolution française*, and the two-year harassment leading to his dismissal from the Collège de France in 1852.

What this study does best is to spotlight the career of the resolute educator and publicist and his writings on France. Focusing on the main narrative themes such as France's unification, Kippur proceeds by noting intellectual influences, by knowledgeably comparing Michelet's ideas with those of his contemporaries, and above all by paraphrasing and quoting from the major historical writings. Throughout, he tends to take his subject's words at face value. Intellectual complexity of the most explicit sort does come through in the examination of the historian's changes of mind regarding the Middle Ages, the church, and nature; Kippur also nicely sorts through Michelet's ambiguous and ambivalent view of "the people." Though stinting in critical comment and examination of less evident themes and images, Kippur's accounts are convenient introductions to the voluminous histories. Books other than the histories and *Le peuple* receive sketchy treatment at best; several, including the *Origines du droit français* and the *Bible de l'humanité*, are not treated at all.

For the reader wanting a brief but factually sound and chronologically complete introduction in English, the book will be useful. Readers of French will still find fuller detail in Gabriel Monod's biography (though only to 1852) and richer interpretation in Paul Viallaneix's classic *Voie royale*. For understanding the importance of the nature books, the reader must turn to the recent works by Linda Orr and Edward Kaplan. Readers will also have to turn to these and other studies to get a sense of the poetic and philosophical richness of Michelet's texts. Recent analyses of his imagination—or his mythic structures—have borne fascinating fruit that is not to be found here (see, for example, Lionel Gossman's excellent 1974 essay). The book ends with a sketch of Michelet's influence on later historiography but does not suggest why he has been of such abiding interest to students of literature and ideas.

A creative writer of Michelet's genius who made such a point of "resurrecting" "dead bones" certainly presents the highest challenge to a biographer. The fact that he left us with so much intimate self-revelation perhaps adds to the difficulty but also offers promising possibilities and raises ex-

pectations. In view of the abundance of Michelet literature, primary and secondary, it seems a pity that he has not been given a fuller "second life."

CHARLES REARICK  
University of Massachusetts,  
Amherst

PIERRE CAYEZ. *Crises et croissance de l'industrie lyonnaise, 1850-1900*. Lyons: Centre National de la Recherche Scientifique. 1980. Pp. 357. 120 fr.

This second volume of Pierre Cayez's *thèse de doctorat* focuses, like the first (*Métiers jacquards et hauts fourneaux* [1978]), on industrialization in the city of Lyons and on investment, in Lyons and elsewhere, of Lyons-based capital. The research for this study is detailed, usually clearly, concisely presented, and fresh, based on public serial archives and private accounting records of individual firms, unexplored until now. One wishes, however, that the CNRS press had seen fit to print at least a summary listing of sources. Topics include trends in major industries (silk, iron, chemical, gas, metal working and machine making, transport, various consumers' industries), changes in product, location, technique, organization, and factor markets in these industries, and "biographies" of individual enterprises. The last illustrate strategies of adapting to change and a varied record of success and failure in doing so.

As compared with the first half of the century, industrial diversification, technological innovation, and the separation of commercial-financial from industrial capitalism were the most striking changes in Lyonnaise industry during the period 1850-1900. The highly speculative character of capital, though now concentrated increasingly in the hands of a few big capitalists and banks, was the main carry-over from the preceding period, the vestige of a primarily commercial orientation of the premodern manufacturing city. This made financing of industrial expansion a risky proposition, even with the facility provided by limited liability, because of high returns or quick repayment demanded by suppliers of capital as a condition of their participation in industrial enterprise. The risk was heightened by the unfavorable conjuncture of falling prices and profits and more intense competition. Cayez emphasizes the variety of strategies taken for survival or expansion in this context: "flight in advance" (expansion to overtake falling profits), often with the help of outside financing; self-financing through retained earnings; regrouping of firms in cooperative associations to minimize competition without sacrifice of autonomy; change of company form; and technical adaptation, orienting enterprise to a more exclusively industrial vocation, as in mechanized factory production of silks.

Cayez's interweaving of conjunctural pressures with strategic response at the firm level, manifesting various entrepreneurial opportunities and styles, is the most subtle and enlightening feature of his study. However, this focus on entrepreneurship gives his analysis of the structural adaptation of the Lyons economy a strongly voluntarist tone. Entrepreneurship is thus given too much of the credit, or blame, for the outcome in the aggregate. The study would have profited from more pointed, rigorous economic analysis, using some of the theoretical concepts and tools of neoclassical economics. In particular, one would like to learn more about the role of the regional economy as a whole—especially the agrarian sector, migration and labor supply, and urbanization and industrialization in satellite towns and cities—in conditioning the investment opportunities and decisions of Lyons's industrialists and capitalists. To be fair, Cayez probably regarded these topics as "pre-empted" by the *thèses* of Garrier, Lequin, Merley, and others. Cayez's study would have had to draw on conclusions of these studies more closely than it did, in the framework of a regional growth or development model, to create the kind of economic study suggested here. As it stands, his study is a mine of valuable information, with some fascinating generalizations about the behavior and performance of capitalism in later nineteenth-century Lyons. These generalizations will have to be articulated and tested in a regional context in subsequent work.

GEORGE J. SHERIDAN, JR.  
University of Oregon

ROGER L. WILLIAMS. *The Horror of Life*. Chicago: University of Chicago Press. 1980. Pp. xiii, 381. \$22.50.

One of the more positive consequences of the much-maligned psychohistory has been the vastly expanded range of what constitutes historical fact—which now includes not only social and economic forces, but childhood experiences, bad mothers, and other events and situations that serve to illuminate the interior life. The unconscious has become a significant part of written history.

Roger L. Williams, in a superbly crafted and well-researched work, has added a further dimension to historical writing by reconstructing the medical histories of five nineteenth-century French writers: Charles Baudelaire, Jules de Goncourt, Gustave Flaubert, Guy de Maupassant, and Alphonse Daudet. All of them shared what one of them termed "the horror of life." If they had been German, one would speak of the presence of *Kulturpessimismus*; as it is, their rejection of nineteenth-century culture and their contempt for developing democracy and increasing social mobility were much more personal and private emotions than conscious

political postures. Indeed, political ideas and ideologies were marginal to their writings. What the five did share, however, was a strong sense of alienation from their society and (except for Daudet) a snobbish contempt for their fellow human beings. Their pessimism was predicated on the conviction that "excellence and virtue are about to be exterminated by gadgets" (p. x).

The three pillars supporting this pessimism and sense of alienation were a general intellectual disgust with the French environment of their time, their personal relationships, and their medical histories. Williams minimizes the first: although a few references to the Paris Commune and the Dreyfus affair appear, these sorts of events barely figure in the biographies. Nor does Williams attempt to provide a sociological analysis of the historical period that would explain the alienation of his subjects. A much greater emphasis is given to personal developments in the lives of the five writers. Although their individual circumstances differed, all of these men lacked the ability to form warm human relationships; they appear emotionally impoverished and flawed. Williams describes their individual medical histories and, incidentally, provides us with rare insights into the practice of nineteenth-century medicine.

Perhaps the most tragic of the diseases described by Williams are those that resulted from syphilitic infections and caused madness in several of his subjects. Indeed, syphilis and other afflictions were both a reality and a metaphor for the interior life. Williams shows how disease and its concomitant pains profoundly affected the lives and work of the five authors under study. Yet he avoids a simplistic medical determinism and successfully integrates his discussion of each man's physical problems with an analysis of his character and writings.

Although far from sharing their values, Williams indicates a very real sympathy for his subjects. He concludes his discussion of Guy de Maupassant by noting that "Madness came to him, yes, but only at the end of his career, and Carl Jung's cruel verdict that Maupassant's works were the delirium of a general paralytic is quite untenable" (p. 272). This work is an important achievement that not only provides a new level of understanding of the writers discussed but whose methodology also opens up new vistas for historical writing.

GEORGE M. KREN  
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ROBERT L. HOFFMAN. *More than a Trial: The Struggle over Captain Dreyfus*. New York: Free Press. 1980. Pp. viii, 247. \$14.95.

As its title indicates, Robert L. Hoffman's new book argues that the Dreyfus affair was far "more than a trial." At stake was not only the issue of assuring



justice in a single case but also the survival of the Republic and of democratic values. For the anti-Dreyfusards, the stakes were the triumph of xenophobic nationalism and the honor of the army.

The familiar story of the long-term political effect of the affair on French politics is retold: the triumph of anticlericalism and secularization, the emergence of Zionism, the breakup of the coalition of the left, the increasing radicalization of some bourgeois intellectuals, and the development of the *Action Française*, which linked nineteenth-century extreme right-wing politics and antisemitism with the Vichy version of the same combination.

If there is little disagreement among scholars on these interpretations, the same cannot be said of the role antisemitism played in the affair. Would Dreyfus have been convicted and re-convicted, and would he have become the center of a major "affair" if he had not been a Jew? During the affair itself the Dreyfusards were divided on the issue. Bernard-Lazare angered many when his pamphlet, *A Judicial Error*, insisted that Dreyfus was prosecuted and convicted because he was a Jew. The organized French Jewish community refused to see the affair as a Jewish affair and minimized the role of antisemitism. This political blindness was faulted by Hannah Arendt (*The Origins of Totalitarianism* [1951]), whose theory of the historical significance of political antisemitism set the tone for much of the subsequent scholarship stressing the role of antisemitism in both the initial prosecution and the affair that followed.

The counterargument was established in 1955 by Guy Chapman's theory that "antisemitism appears to have played no part in the case until Drumont and the Assumptionists took advantage of the fact that Dreyfus was a Jew." Douglas Johnson (*France and the Dreyfus Affair* [1966]) and others have continued this line of argument, the most recent being Allan Mitchell ("The Xenophobic Style: French Counterespionage and the Emergence of the Dreyfus Affair," *Journal of Modern History*, 52 [September, 1980]): "Of the antisemitic ramifications, once Dreyfus was accused and convicted, there can be no question. But . . . the emergence of the Dreyfus case should be attributed less to racism than to xenophobia."

Unfortunately, Hoffman's book brings us no closer to a definitive judgment on this question. In fact, his position is equivocal. On the one hand, he argues that the affair was provoked and sustained by the desperate need and desire to protect the honor of the army and of France and that antisemitism was not a cause; "... the role played in the Affair by antisemitism has been misunderstood. Alfred Dreyfus was not persecuted because he was Jewish: the decisive factors in actions by officials against him would have been little if any altered had he been a Gentile. They had motivations that

were more compelling than any prejudice in determining their conduct. The need of army officers to be right and to protect themselves and the Army, the desire of Ministers to avoid both domestic and international political losses" (p. 200). But eight pages later, he begins a sentence, "When Dreyfus was denied justice because he was Jewish . . ." (p. 208, compare p. 210).

Another related inconsistency is the question of whether French political antisemitism survived the end of the Dreyfus affair. Although Hoffman says that antisemitism was *not* incorporated into a continuing political program (pp. 197-99), he then contradicts this by describing the *Action Française* as a political movement with antisemitism as an integral part. He even sees this political movement as the link between the political antisemitism of the Dreyfus affair and that of Vichy: "It has been said with justice that the authoritarian Vichy regime in France, 1940-1944, was the 'revenge of the anti-Dreyfusards,' because it embodied so much of the anti-Dreyfusard spirit . . . the Action Française had done much to make the continuity possible" (p. 33).

By emphasizing the political failure of antisemitism in the period between the Dreyfus affair and Vichy, Hoffman ignores its real impact. As another scholar has recently pointed out, "[in 1911] the anti-Rothschild propaganda threatened to explode into a full-scale outburst of antisemitism . . . the threat was perceived as real" (Paul Mazgaj, *The Action Française and Revolutionary Syndicalism* [1979], p. 216).

As an introduction to the France of the Dreyfus period, this volume may be recommended to students. Although written in a more scholarly vein than earlier popular studies by David Lewis and Nicolas Halasz, *More Than a Trial* reads very smoothly and reminds us once again that the Dreyfus affair remains "the detective story par excellence" (David Lewis, *Prisoners of Honor: The Dreyfus Affair*, p. vi).

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WILLIAM R. KEYLOR. *Jacques Bainville and the Renaissance of Royalist History in Twentieth-Century France*. Baton Rouge: Louisiana State University Press. 1979. Pp. xxvi, 349. \$25.00.

If readers of this review have actually read the historical work of Jacques Bainville, they probably did so to prepare for doctoral examinations. Not even William R. Keylor's excellent book will attract many new or old readers to Bainville, but it will allow us to see him in his real importance. He was not a particularly good historian. Rather, as a disciple of Maurras's royalist *Action Française*, he wrote a "usable past," and he was so successful at it that he be-



came probably France's most widely read historian. Through that popularity he was the *Action Française's* bridge to a much wider audience. The French view of the past in the first half of the twentieth century was doubtless that of Bainville, royalist and Germanophobe. Keylor does not take enough pains to demonstrate this beyond citation of publication figures and personal recollections, but he convinces the reader of it just the same.

Bainville led a remarkably dull life. Rarely a political activist, seldom appearing in police reports, he lived out his years in a happy marriage and with pen almost constantly in hand. A bourgeois republican upbringing and a hatred of history as a school-boy hardly prepared him for being either a royalist or a historian. He came to royalism through Maurras via Barrès. He came to history through the writing of columns on foreign policy for royalist newspapers and the books that came out of the columns. The *union sacrée* created by World War I permitted Bainville to reach beyond his royalist audience to a wider one with his *Histoire de deux peuples*, published by Fayard in 1915. Its success and the subsequent popularity of such works as his *Histoire de France* (1924), *Napoleon* (1931), and *Histoire de la Troisième République* (1935) allowed Bainville to place some distance between himself and the Maurrasians, a distance that made him the first *Action Française* member to be ushered into the *Académie Française*. Those three books alone had combined sales of over 600,000 copies.

Given this life, Keylor's book is primarily a chronological analysis of Bainville's historical writings. One is initially surprised that Keylor does not bring to this work the analytic power of his *Academy and Community*, until one realizes that Bainville was not that kind of historian. Bainville's purpose was to explain to his bourgeois audience the pasts of particular presents with *politique d'abord*. Because those presents changed, his work maintained few constants, notably a hostility to democracy and Germany. Those threats were best checked by balance of power international politics and royalist French governments unswayed by "public opinion." Frequent inconsistencies arose as Bainville changed his views to fit changing times. For example, Anglophobia turned to wartime Anglophilia and back again, initial admiration for Mussolini became disillusionment, and prewar opposition to French colonialism became postwar support for it. Keylor explains these inconsistencies by pointing to Bainville's "realism." Through this perceived realism and relevance, Bainville was a principal guide for France's elite. After his death in 1935, sale of his work even enjoyed a revival during World War II and the liberation.

Keylor's analysis of Bainville's work is quite good and is especially valuable, given Bainville's popularity, in revealing the values that that work im-

parted to its *bien pensant* readers. On other matters he sometimes does not say enough or is not quite convincing. Keylor gives a fine analysis of Bainville's "spirited defense of French capitalism," but he tantalizes the reader with the suggestion that investors could become rich by following Bainville's advice in *Après la Guerre: Comment placer sa fortune* (1919) and his weekly columns in *Le Capital* without telling what the advice was. Keylor likewise buries in a footnote Bainville's antisemitism. The latter might have been treated more fully in the text to see if Bainville, in this matter as in others, was a bridge from the *Action Française* to a wider audience. Readers convinced by William Irvine's recent linking of the right's fear of revolution with appeasement will have trouble with Keylor's treatment of Bainville's views of the Russian alliance and the emerging Popular Front.

Nonetheless, this is an important and fine work of French intellectual history, and Keylor is additionally fortunate in receiving from Louisiana State University Press its continuing high performance in book production, even to having the footnotes at the bottom of the page.

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JONATHAN BROWN and J. H. ELLIOTT. *A Palace for a King: The Buen Retiro and the Court of Philip IV*. New Haven: Yale University Press. 1980. Pp. xvi, 296. \$29.95.

After reading *A Palace for a King*, one can begin to identify with Shelley's immortalized "traveler" who discovered the ruined statue of Ozymandias, "king of kings." "Round the decay/Of that colossal wreck, boundless and bare/The lone level sands stretch far away." In an effort of historical re-creation equal to Shelley's poetic powers, Jonathan Brown and J. H. Elliott have restored in the pages of this book another "colossal wreck" to the grandeur and dignity it once possessed. The wreck is the Buen Retiro, an urban pleasure palace built in Madrid during the 1630s for the enjoyment and glorification of Philip IV. Some two centuries later, in 1813, it was abandoned in ruins by Napoleon's troops. Brown and Elliott recount in painstaking detail the construction and furnishing of the Retiro and describe its use from the moment it emerged from the chrysalis of the royal apartment annexed to the Church of San Jerónimo to the downfall in 1643 of Philip IV's favorite and prime minister and the Retiro's chief inspiration, the Count-Duke of Olivares.

The book is a major scholarly achievement and testifies to the value of interdisciplinary collaboration. The Retiro, built by courtiers for purposes of

court display, was constructed at a time when Spain was hopelessly mired in the quagmire of the Thirty Years' War, its finances depleted, its economy depressed, and its political leaders, particularly Olivares, criticized and condemned. Elliott, the historian, deftly pieces together the circumstances in which the Retiro was conceived and executed: in the process he recaptures the psychology of Philip IV's court. The Retiro was also of architectural significance, although as a building it was by no means universally admired. In addition it housed what was undoubtedly the largest and finest "modern" art collection of its day. Brown, the art historian, masterfully explains the genesis of its form and elucidates the stylistic and thematic attributes of its holdings.

These insights are gracefully interwoven, their combined effect becoming most compelling in the chapter devoted to the description of the throne room, the Hall of Realms. Decorating this elongated hall were ten canvasses by Zubarán depicting the Labors of Hercules; Velázquez's famous equestrian portraits of Philip IV and Isabella of Bourbon, their son Baltasar Carlos, and their predecessors Philip III and Margaret of Austria; the coats of arms of the realms comprising imperial Spain; and twelve battle scenes commemorating contemporary Spanish victories. Of the latter, those singled out for detailed analysis are Velázquez's *Surrender of Breda* and Maino's *Recapture of Bahía*. Seen within the context of the Hall of Realms, both paintings reveal how effectively art was used for propaganda. They served as self-congratulatory chronicles of contemporary history and as visual paeans to Philip IV, "king of kings."

The real hero of *A Palace for a King* is not Philip, however, but Olivares. The authors describe the Retiro as "a projection in three dimensions of the personality and policies of the count- duke" and seem anxious to explain, almost to justify, his preoccupation with it. They place his portrait opposite the title page. Their implied reason for terminating the detailed description of life in the Retiro with his downfall is that "on the surface at least, the festivities and amusements in the Retiro during the 1650s were much like those of the 1630s, but the supreme master of ceremonies the count- duke was gone." This single-minded focus is regrettable, not because the case for Olivares as the mastermind of the Retiro is unconvincing, but rather because Philip, for whom the palace was built, becomes too much the secondary figure. He continued to reign and to use the Retiro until 1665, but the book misses the opportunity to penetrate the obscurity still surrounding the Spanish court during the latter half of his reign.

But the emphasis given to Olivares by no means detracts from the overall quality of *A Palace for a King*. Like Shelley's *Ozymandias*, the book will re-

mind readers for generations to come that magnificent ruins can be made magnificently instructive.

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OSKAR GARSTEIN. *Rome and the Counter-Reformation in Scandinavia*. Volume 2, 1583-1622. (Scandinavian University Books.) Oslo: Universitetsforlaget; distributed by Columbia University Press, New York. 1980. Pp. xii, 626. \$40.00.

In this volume (the second of four), Oskar Garstein acquaints us with the efforts of the Counter Reformation in Scandinavia in the period 1583-1622. Emphasis is upon a tiny number of dedicated Roman Catholics, mainly Jesuits, who struggled to win converts and gain concessions that might, in time, bring Denmark-Norway and Sweden-Finland back to the "true faith." Their proposals and reports to superiors in Rome and to each other are cited at length; the record of their movements and the accounts of their occasional successes and general failure are supplied us. This focus upon men like Antonio Possevino, Laurentius Norvegus, Orazio Malaspina, and Petrus Petrosa is not, however, allowed to distort the general story. The measures taken against these would-be subverters of Scandinavian Protestantism are reported dispassionately and at length, and the book becomes an only slightly slanted recapitulation of the religio-political history of the period. Quite naturally the interesting complications revolving around the career of Sigismund III, the Catholic king of Catholic Poland and Protestant Sweden, are explored at length. The even more hopeless efforts of Laurentius Norvegus in Denmark-Norway, meanwhile, receive attention proportionate less to their importance than to Garstein's long-standing research interests.

Considering the trends and costs in publishing, this book may turn out to be part of a "definitive work" on the Counter Reformation in Scandinavia. That is unfortunate because, even if it proves to be the best English-language history on the subject, it could have been better. It is certainly scholarly. More than a third of the volume is small print notes, bibliography, and index. There are lengthy passages of untranslated Latin to titillate the knowledgeable (although Garstein evidently believes his readers less capable of dealing with living languages like Swedish or Polish). Based not only (as advertised) upon the materials in the Kolsrud Collection in Oslo but also upon just about everything one could imagine relevant, the book is a mine of information.

The reviewer, nevertheless, is uneasy. The seemingly haphazard decisions on translation or transliteration are a minor annoyance. More importantly, the book's organization is disjointed to a

degree not justified by the story told; redundancies and repetitions abound (perhaps due to piecemeal preparation and an eye on succeeding volumes). Most distressing, however, is the fact that even the principal actors in this deeply emotional drama rarely emerge as anything more than stick figures. We are given many a barebones *vita* and occasionally somebody's *apologia*, but master plotters, missionaries, and even martyrs remain remarkably bloodless. Why they bothered to risk their necks is never clear. How in the world they went about persuading anyone to abandon one faith for another is hardly touched upon (although the prospect of heavy penalties seems to have cooled a lot of ardor). There is very little evidence about how the common people in Scandinavia felt. Finally, the Rome that figures so prominently in the title seems hardly entitled to the billing, so remote was its control. But then perhaps that is what Garstein is trying to tell us.

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D. G. KIRBY. *Finland in the Twentieth Century*. Minneapolis: University of Minnesota Press. 1979. Pp. x, 253. \$19.75.

The popularity of the term "Finlandization" implicitly confirms D. G. Kirby's dictum that recent Finnish history "is little known, even to historians, outside Finland" (p. ix). A number of specialized studies have been produced by American and British scholars, but a synthesis has been lacking. John H. Wuorinen's large survey history (1965) is badly marred by the author's championship of discredited partisan interpretations long since abandoned by Finnish historians; Eino Jutikkala (1979) treats the twentieth century as little more than an epilogue; and Lauri Puntila's post-1809 political history (1975) is a flawed and elementary text. Kirby's book fills a gaping void.

While emphasizing political history, with brief excursions into social and economic affairs, Kirby's main theme is the transformation of Finland in this century from a backward agrarian society on Europe's periphery into a modern state. His chapters cover the period of Russian oppression (notably from 1907 to 1917), Finnish independence and civil war (1917–20), the turmoil of the 1920s, when Russophobia became "virtually synonymous with patriotism" (p. 80), the resilience of the political system in the 1930s when threatened from the right, the failure of foreign policy before 1948, the "repair on the body politic" effected after World War II, and the changes wrought in the past two decades, when "the old high priests of Finnish peasant nationalism [were] replaced by the new presbyters of the Kekkonen orthodoxy" (p. 209).

Kirby's presentation reflects up-to-date scholarship, but there are some dubious conclusions, such as his claim that the Finnish-Soviet crises of the 1950s and early 1960s were due mainly to foreign trade factors. This fails to explain the Note crisis of 1961, and it lets the Social Democratic party off the hook. The party's split, Väinö Tanner's return as chairman (1957–63), its militant anticommunism, and its strident pro-Western rhetoric offer a more logical explanation. Kirby might have consulted the memoirs of Karl-August Fagerholm, whose cabinets were toppled in a couple of those crises.

The level of objectivity is generally high, but on one occasion Kirby takes a cheap shot that does him little honor. Relying on an article published in 1958, Kirby implies that Puntila remains wedded to the view that the civil war of 1918 was a war of liberation (p. 51). As Kirby well knows, that view was abandoned shortly thereafter by almost all Finnish historians, including Puntila, as seen in the 1963 edition of his political history.

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MAXIMILIAN LANZINNER. *Fürst, Räte und Landstände: Die Entstehung der Zentralbehörden in Bayern, 1511–1598*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 61.) Göttingen: Vandenhoeck und Ruprecht. 1980. Pp. 454. DM 92.

Bavaria was one of the first German principalities in which the dualistic division of government between princes and estates was resolved in favor of the absolutistic authority of the duke. This study, based on a dissertation presented in 1977 at the University of Regensburg, is a useful addition to the growing collection of books on the development of central government in the early modern Wittelsbach state.

Eschewing traditional approaches that have emphasized institutional history, Maximilian Lanzinner focuses instead on the social and economic structure of Bavarian officialdom. He does this by analyzing the interaction of the three dominant forces involved in this development—the Bavarian dukes, their councilors, and the estates. The years 1511–98 are chosen for this study since it was during this period that the balance clearly tilted in favor of the princes as the estates increasingly lost power while officialdom was streamlined for more effective central government.

The book is divided into five major sections. Part 1 briefly traces the development and organization of the leading governmental councils in the sixteenth century. Lanzinner shows how the constantly growing financial needs of Dukes Albert V and William V played a decisive role in this proc-

ess. Part 2 discusses the councilors' personal qualifications and the conditions under which they served—their class origin, education, religion, remuneration, advancement, social position, and special privileges.

Part 3, the heart of the study, analyzes the social structure of the various councils. In order to understand the members of the central bureaucracy as a social group, Lanzinner moves beyond a simple noble-burgher classification to a more specific differentiation within each class. Particularly helpful here is his analysis of the upward mobility of non-nobles utilizing marriage, property, and ennoblement records. Finally, in Part 4, Lanzinner analyzes the social and political overlap between the estates and the central government. The study concludes on pages 289–421 (part 5) with a listing of all secretaries and councilors that served in the Munich government in the years 1511–98. For each, this listing includes personal data about the individual's family, education, service, and property holdings together with some bibliographical information. This directory in particular should be of great service to others wishing to do research in the area.

BODO NISCHAN  
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KASPAR VON GREYERZ. *The Late City Reformation in Germany: The Case of Colmar, 1522–1628*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung für Abendländische Religionsgeschichte, number 98.) Wiesbaden: Franz Steiner Verlag. 1980. Pp. xiv, 236. DM 68.

Interest in the Reformation as an urban movement has not faded since Bernd Moeller sparked it in 1962 with his *Imperial Cities and the Reformation*. A few critical questions have dominated the debate (to which Steven Ozment, Hans-Christoph Rublack, and Thomas Brady have made notable contributions): Did the urban tradition create its own momentum propelling cities to the Reformation? Were considerations of class paramount in each case? Did the movement originate in the populace or was it imposed from above, or is it best understood as a joint endeavor of popular groups and patrician councils? Was the Lutheran message heard in the city as a call to religious renewal, moral regeneration, social reform, or all of these?

Kaspar von Greyerz, a young Swiss historian writing in English, has now posed another question. If, instead of concentrating, as all recent scholars have done, on cities where the Reformation occurred in the first half of the sixteenth century, we examine towns where it took place in the 1570s and later, what patterns emerge? For his answer he

turns to the Alsatian city of Colmar whose archives yield a richly detailed picture of a course of Reformation that veered in fits and spurts from an abortive popular uprising in the 1520s to the official establishment of Lutheranism in 1575, Calvinism in 1603, the Counter Reformation in 1628, and Lutheranism again in 1632. In each instance, after 1525, reform was introduced from above. In each instance the change clearly represented the interests of the city's ruling oligarchy, a tight-knit group of bourgeois patricians joined in the ownership of commercial and landed wealth and control of the city council. Greyerz thus supports Brady's interpretation of the Reformation in Strasbourg as mainly the work of dominant social groups holding on to their ruling positions. He finds, however, that "only by the second half of the sixteenth century . . . had the oligarchical structure of urban regimes become so pre-eminent that, in introducing the Reformation, city councils could act essentially on their own behalf, neither in response to popular pressure nor in anticipation of it through their own actions." A curious manifestation of this independence of action is the fact that, throughout the period, Protestants were the minority in Colmar, whose largely rural population remained mostly Catholic.

Comparison of the Colmar Reformation with events in Essen, Aachen, Haguenau, and Aalen reveals a "Pattern of the Late City Reformation in Germany." The late Reformation took place in a period of general urban decline, one response to which was the strengthening of oligarchy in many cities. As a "magistrates' reform," the late city Reformation adopted the authoritarian methods of the Counter Reformation. In no way was it an outgrowth of indigenous spiritual roots; none of the cities boasted an inspiring municipal reformer. Very little communal reform (of education, poor relief, and so forth) took place. A political movement, the late urban Reformation must be seen as an attempt to free cities from outside influences—Habsburg Austria in the case of Colmar and Haguenau. For this purpose Calvinism seemed at times a more useful variety of Protestantism than orthodox Lutheranism.

While admitting the relative unimportance of the late urban Reformation—by 1550 the action had shifted to the princely territories—the author claims for his subject a place in history owing to its distinctness from the early Reformation in the cities. He makes his case by thorough research and a wide comparative frame of reference. His book makes a useful addition to the literature on urban history in the time of the Reformation.

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WINFRIED SCHULZE. *Bäuerlicher Widerstand und feudale Herrschaft in der frühen Neuzeit*. (Neuzeit im Aufbau, number 6.) Stuttgart: Frommann Holzboog. 1980. Pp. 344.

Volume 6 of the series "Neuzeit im Aufbau" concentrates on peasant resistance against the feudal order in the Holy Roman Empire from the time of the great Peasants' War to the French Revolution. Scholars have long ignored the German peasantry in the early modern period, assuming that the defeat of the rebels in 1525 meant the destruction of the peasants' social-revolutionary dream of freedom. Winfried Schulze shows that this dream lived on in the form of legalistic resistance.

The author argues that the period 1525–1789 saw a gradual legalization of social conflicts, which took a "latent," "manifest," or "violent" form. Latent resistance consisted of passive refusal to render dues and services owed to the immediate overlord. Manifest resistance involved judicial procedures by peasants against their lords. Violent resistance, or open warfare, occurred less often than the other types; for this study, clearly manifest resistance is of greatest importance.

It is Schulze's thesis that the German peasantry's passion for lawsuits served to legalize social conflict, to weaken the petty nobility, and to strengthen the great territorial princes. The centralizing prince wanted a prosperous populace for a stable tax base and was therefore willing to use his courts to block increased payments demanded by petty local magnates. The imperial government was attempting to establish supervisory jurisdiction in the areas of taxation and justice, especially within the smaller territories. Thus the disgruntled peasant could thwart his overlord's attempts to impose new obligations by legal action either in a territorial or an imperial court. For financial reasons petty magnates most needed to expand their peasants' obligations, but they were also the most vulnerable to an appeal to a higher court. Such appeals weakened the position of the lesser nobles, strengthened the hand of the centralizing territorial state builder, and contributed to the destruction of the last vestiges of the feudal-manorial social structure.

Traditionally, scholars have treated the peasants' role in the construction of the territorial state in terms of the impact of the great revolt of 1525. At that time the rebel armies destroyed hundreds of castles of minor nobles. This seriously weakened the ability of this class to oppose the great princes. Schulze, however, has now pointed out that peasant judicial resistance also contributed to the development of princely absolutism.

The latter half of the volume consists of a variety of printed sources. Documents readily available elsewhere are not included, even though this means that sources on the best-known revolts in the early

modern empire are omitted. Unfortunately there are no maps, the addition of which would greatly clarify the discussion of regional peasant activity. The bibliography is extensive and judiciously selected, but it is divided into sixteen separate categories that make it cumbersome to use. In sum, Schulze's volume is a fine research and teaching tool that provides a detailed supplement to a standard account like Günther Franz's *Geschichte des deutschen Bauernstandes*.

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PETER BLICKLE, editor. *Aufbruch und Empörung? Studien zum bäuerlichen Widerstand im Alten Reich*. Munich: Verlag C. H. Beck. 1980. Pp. xi, 320. DM 98.

With this first product from what promises to be a studio for peasant research underwritten by Volkswagen, Peter Blicke has returned "peasant revolt" studies to their honored foundations in traditionally liberal history. He has validated the stance of his mentor, Günther Franz, and has supplanted Franz with a more capacious methodology.

This work is extremely systematic. At the outset, Peter Bierbrauer surveys current literature on European agrarian protest in order to emerge with a research methodology usable for systematic study in the "case histories" that follow. These comprise report and analysis of peasant actions in three discreet South German localities: Rottenbuch in Bavaria, by Renate Blicke; the Habsburg *Vogtei* of Triberg in the Schwarzwald, by Claudia Ulbrich; and the Prince-Abbey of St. Gall in the Swiss Confederation, by Blicke. In each instance, a chronological narrative of the fourteenth through eighteenth centuries is recapitulated systematically according to the schema developed in the literature review.

The book finishes with an effort by Blicke to distill some overall generalizations. A useful working hypothesis is that peasant actions arose when an existing relationship of privileges and rights was stressed by the ruling authority under conditions (1) in which the lord's authority was real but unsatisfying egotistically or fiscally and (2) in which the peasantry had a constituted role in governance. Peasant objectives were to reduce or limit obligations, to enhance political participation, and to increase individual personhood. The peasantry actually shifted its legitimizations over time, keeping with the trend away from customary law and toward positive and written law. The protests always were carried out by the leadership stratum of and chosen by the peasantry.

At this point, before looking at Blicke's "Consequences," some generalizations about his accomplishment are in order. Insofar as his objective is to



devise a research method for more or less standardizing localized study into peasant protest, his two-part, five-stage model is quite workable. Its use could ultimately facilitate his larger objective of providing empirical materials for regional, inter-regional, and even European-wide generalizations about peasant protests. In this context, it is important not to dismiss Blickle's labor as merely a counter-methodology to the Steinmetz theses and the proliferation of Marxist studies into the "early capitalist revolution." Blickle's contribution is more than that, on the condition, however, that he is allowed his methodological axioms. These are (1) careful selection of test localities to achieve a near paradigm of landlord-tenant typologies; (2) traditional, archival documentary sources subjected to the most severely modern scientific-historical analysis.

Granted these, Blickle's achievement is sizable. It is a reassertion on a much vaster scale of Günter Franz's ironbound assertions. Peasant revolts were political. They were conservative regarding the old law and were progressive toward human freedom, participational politics, and resistance to tyranny.

It is reassuring to find that the venerable and honored foundations of traditional, liberal historical study can again support lofty historical vision. It is good to learn that the document is still valid and need not be supplanted by the methodologically demonstrable primal urge to protect family property, as shown by David Sabean, nor by complicated manipulations of hypothetical structures from cultural anthropology, as proffered by Heide Wunder. Marx and Engels reacted against the dark side of nineteenth-century liberalism. The vehemence and volume of their twentieth-century historian adherents have blown traditional (and overcontented) liberal historians into well-deserved disarray. Blickle's reassertion of the liberal tradition by way of profound research and exacting method is commendable. His conclusion as to the consequences of peasant protest, that it seems possible as well as appropriate to establish a historical role in the old regime for this 80 to 90 percent of its population, is welcome. Blickle offers the good old wine in new bottles.

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LOYD E. LEE. *The Politics of Harmony: Civil Service, Liberalism, and Social Reform in Baden, 1800-1850*. Newark: University of Delaware Press or Associated University Presses, London. 1980. Pp. 271. \$22.50.

The role of bureaucrats in socioeconomic reform and state building in nineteenth-century Germany has long been a major subject of research, and historians have debated whether Prussia's bureaucratic

reformers were really liberals or conservatives. Baden, however, has always been portrayed as a leader of liberalism. Loyd E. Lee has set out to test Baden's liberalism by asking "whether the Prussian experience can be considered normative for other German states" (p. 11). Consequently he focuses, as his subtitle suggests, on the reform efforts of Baden's bureaucrats, relying most heavily on the debates in the diet and official memoranda in the government gazette and state archive.

Lee divides the half century into five periods: (1) constitutionalism and state building to 1819; (2) inaction and disagreement from 1819 to 1831; (3) the heyday of liberal reforms under the officials Ludwig Winter and Karl Nebenius from 1831 to 1839; (4) a brief conservative reaction to 1843; and (5) a period lasting through the revolution of 1848, in which Nebenius, Friedrich Rettig, and other officials sought more to protect existing reforms than to initiate new ones. Throughout, Lee argues that an almost closed caste of civil servants sought to impose liberal reforms on a reluctant or resisting population while simultaneously establishing and entrenching themselves as Baden's most important privileged elite. This civil-service liberalism, however, was inconsistent and often equivocal as leading figures changed their positions in response to new political or social conditions, conclusions not very startling and fairly consistent with the arguments of Mack Walker, Leonard Krieger, and others.

Unfortunately, the book suffers from several flaws. The promised comparison with Prussia is simply not made. Lee assumes that the Prussian experience was uniform and is known to his readers. He does not confront the influence of the Prussian reforms, the Napoleonic experience, or the French bureaucratic model. Liberalism is identified not with an ideology or a party program but with whatever the civil-servant reformers wanted, and figures like Karl Rotteck are treated solely as civil servants, not as intellectuals. Lee finds it paradoxical that civil servants could oppose specific government proposals and then later claim to lead the state, but the instances he cites are explained by the change in an individual's own situation as he moved from being a local official to serving as a minister or deputy minister. There is too much reliance on parliamentary debates and too little effort at following the actual application of the reforms. Finally, the book suffers from poor editing and proofreading. Lee's prose is often murky, and there are many technical errors: a missing footnote number in the text on page 60, a statement about ending "federal" dues when presumably feudal is meant (p. 52), an unclear sentence with half of a set of quotation marks, suggesting a missing line (p. 142), to name but a few.

JEFFRY M. DIEFENDORF  
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ANTHONY J. LA VOPA. *Prussian Schoolteachers: Profession and Office, 1763-1848*. Chapel Hill: University of North Carolina Press. 1980. Pp. x, 220. \$19.50.

Out of the student unrest of the 1960s came a new scholarly interest in the history of education; would it be hazardous to predict that the student "vocationalism" of the 1970s will spur interest in the history of professions? That interest is already awakening on both sides of the Atlantic. Anthony J. La Vopa's new book is clearly more about educators than education, less about the liberation of pupils from illiteracy than the "emancipation" of the schoolteachers from traditional occupational misery.

This "emancipation" involves only the category of teacher to which La Vopa restricts himself, the "schoolmaster" in rural and, to a lesser extent, municipal primary schools. Teachers in higher and usually more urban schools such as the *Gymnasium* were normally university graduates and therefore already part of the middle-class elite. The "schoolmaster" was either poorly trained or, at best, treated to a short course of "normal" training in a special barrackslike *Seminar*. (This distinction incidentally persisted well into the twentieth century.) The author shows quite clearly and backs up with considerable documentation the almost Sisyphean struggle of the Prussian "schoolmaster" to become a highly trained state official and thereby wriggle out of a humiliating dependency on spiteful local peasants, pinchpenny town fathers, haughty pastors, and inspectors. At the same time, professional status might signal the end to the grinding poverty of his "trade," by basing his income on a state salary rather than hard-to-collect local fees. La Vopa also argues convincingly for the continuity of an ideology backing the schoolteachers' claims that derived from the crude social engineering of the Enlightenment and had been in part imposed on them by the Prussian state. Their tragedy down to 1848 was that the state never carried through consistently and in the end blocked the legal and financial means to create a truly professional schoolteacher class. It is a story of aspiration and conflict with few triumphs, and it will provoke thought from any who still believe that Prussia was very far ahead of all other European states in primary education during this period.

One can of course question the degree to which modern professional consciousness was emerging before 1840 in Germany; one seeks vainly for a comprehensive discussion of changing "professional" categories in this brief book. Occasional vagueness and repetition sometimes detract from an otherwise clear style. Much of the sparse statistical information is regrettably given in footnotes instead of tables. Nevertheless, this remains an original and

valuable contribution to the self-understanding of one of Europe's most interesting and significant occupational groups in the nineteenth century. As a bonus, it also gives occasionally fascinating glimpses into the social structure and behavior of rural and small-town society in Prussia.

CHARLES E. MCCLELLAND  
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JEFFRY M. DIEFENDORF. *Businessmen and Politics in the Rhineland, 1789-1834*. Princeton: Princeton University Press. 1980. Pp. xiv, 401. \$22.50.

Jeffry M. Diefendorf's study considers the activities of the small groups of merchants and manufacturers in the three cities of Aachen, Cologne, and Crefeld who attempted to influence government economic policies in their favor through direct petition and service in semi-official institutions such as chambers of commerce, industrial courts, and labor arbitration boards. Diefendorf argues that such activities constituted a "political education" for Rhenish businessmen. The French Revolution, in the form of the French army, ended the Rhenish old regime and with it the parochial isolation of local communities. Under Napoleon especially, businessmen experienced membership in a broader national community for the first time. The consequent broadening of their horizons prepared them for their later experience under Prussian rule. Initial hesitancy toward the Prussian government transformed itself into positive acceptance; businessmen became "lay bureaucrats" serving in a system that considered their interests and generally satisfied their needs.

Although Diefendorf is describing a process of modernization, his actors move not from particularism to universalism, but from localism to a sort of incipient nationalism. More could have been made of this, but Diefendorf's hostility to theory (pp. 12-13) leaves him dependent on a traditional narrative approach, in which two traditional interpretations remain confusingly intertwined. On the one hand, his positive evaluation of the Prussian government's methods and policies (for example, pp. 261-68, 313-33) seems to support the old German nationalist view that saw German unification as the natural consequence of economic development and national consciousness nurtured by Prussia. On the other hand, Diefendorf is concerned in the old Anglo-American manner with the German "problem" and especially with the alleged failure of the middle class to fulfill its historical role. He argues that the Prussian system's very success defused any desire businessmen may have had to seize power for themselves and thereby begin the democratization of German society—despite claims to agnosticism, he

appears to believe that this is what happened in England and that English developments provide a normative standard against which other nations can be judged (compare, for example, pp. 6–12 with pp. 355–56).

There are other problems as well. The key term “political education” is not well defined. It cannot simply mean sophistication, for the elites of urban communities possessed a long and complex history of local conflicts and external relations before the French Revolution. The old empire was a broader community in some of the same ways as France and if anything was more complex in structure. The local elites rapidly adjusted their vocabulary to appeal to the French, and again to appeal to the Prussians, but their interests and arguments changed slowly if at all. Cologne fought successfully for thirty-seven years to preserve the transshipment duties inherited from the late medieval period, and, when their loss could be delayed no longer, pressed for monetary compensation and moved to monopolize the new tugboat trade on the Rhine.

How content were Rhenish businessmen with French, and later Prussian, rule? Within the confines of narrative history it remains difficult to say. The Rhenish commercial and industrial elite showed little inclination to revolt, and, as Diefendorf emphasizes, members of the elite participated willingly in any institution that promised access to influence over government policy. As other historians have emphasized, however, these same individuals protested continuously against government policies that threatened to undermine the position of the urban communities or the position of the elite within those communities. The protests of course became stronger after the date on which Diefendorf chooses to close his study.

The most detailed histories of policy decisions will not resolve such a debate unless linked to analysis of real economic and social conditions. What influence did central government policy actually exert on Rhenish society? Diefendorf’s economic analysis is largely confined to tautologies such as “the real solution to the post-Napoleonic recession was renewed growth” (p. 315). He does not consider changes in social relations at all and therefore does not ask, for example, how the “businessmen” who demanded “harsh police regulations, the means of forming good workers” from the French in 1800 (pp. 164–65) may have differed from their old regime predecessors or from their Restoration successors who called in Prussian troops to suppress labor protests in 1828 and 1830 (pp. 275–76). It seems likely that in the Rhineland, as elsewhere during the early phases of economic development, traditional local communities fractured along class lines, with an entrepreneurial elite using the power of a new central government to maintain and extend its

domination over an expanding and discontented working class. Some such hypothesis might illuminate the changing relations between bureaucrats and businessmen in the Rhineland and even suggest more fruitful ways of looking at the German “problem” itself. Diefendorf offers an abundance of administrative detail, but the narrowness of his approach leaves the meaning of that detail obscure.

FRANK B. TIPTON, JR.  
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CHRISTOF DIPPER. *Die Bauernbefreiung in Deutschland, 1790–1850*. (Urban-Taschenbücher, number 298.) Stuttgart: Verlag W. Kohlhammer. 1980. Pp. 215. DM 16.

This short book achieves a great deal in a lucid and compressed manner. Based upon secondary sources it provides a description, analysis, and evaluation of peasant emancipation in Germany.

The introduction considers the historiography. Historians have concentrated excessively on Prussia. The still influential idea of “revolution from above” simplifies and exaggerates the role of the state. More recently, both Marxist and non-Marxist historians have linked emancipation to the peculiar, authoritarian route Germany took toward capitalism. But it is difficult to link emancipation either to particular class interests or to general economic changes. Christof Dipper suggests that the ideas of “partial modernization,” of the peasantry as community, and of the active peasant contribution to emancipation can assist analysis.

Chapter 1 looks at the situation on the eve of emancipation. Emphasis is placed on population pressure and the changes this had already produced, such as a growth in the extent of pauperism. Chapter 2 outlines the process of emancipation state by state. Complexity and variety are the striking features. Dipper argues convincingly that the major influence was the initial condition of agrarian society. Chapter 3 analyzes systematically the process of emancipation. Changes in definitions of property and in the distinction between public and private are linked to changes in the character of the state. Pauperism or capitalist agriculture (and, in turn, industrialization) cannot be closely linked to emancipation. The transfer of resources that emancipation involved is considered, as are the trends in the burden of debt and taxation. Chapter 4 looks at the peasant contribution to emancipation. The main concern is with peasant unrest. Partial reform could create dissatisfaction in areas that lagged behind and that could, as in 1848, lead to disturbances that could produce further reform. Peasant expectations had become greater by 1848, and it is also clear that different groups of peasants had

rather different expectations. But this is largely uncharted territory.

The final chapter puts the German case into a European context. The dualistic nature of the German case—producing both a vigorous peasant stratum and a class of noble landowners with great estates—is convincingly linked to regional differences in agrarian society at the outset of emancipation. I cannot see, however, that the notion of “partial modernization” is very useful. Dipper himself shows that there is no “normal” or privileged path to modernity, and anyway it is difficult to identify some definite end-state that could be labeled modern. Apart from providing an overview of the historiography and history of peasant emancipation, Dipper clearly shows how inadequate are existing general interpretations. His own ideas about partial modernization and peasant community represent interesting but as yet unproven alternative approaches. To have done all this in so short a compass is a considerable achievement.

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WILLIAM W. HAGEN. *Germans, Poles, and Jews: The Nationality Conflict in the Prussian East, 1772–1914*. Chicago: University of Chicago Press. 1980. Pp. ix, 406.

William W. Hagen's work represents the first stage in a process of American scholars developing their own understanding of nationality conflict in East Central Europe. This stage's key characteristic is the assimilation of the local viewpoint by an American graduate student visiting the remote part of the globe he is studying or sitting at the feet of an émigré professor. In Hagen's case, his book is deeply influenced by the Poznań professors (Jakóbczyk, Topolski, Trzeciakowski) as Hagen's statement of his book's purpose reveals: “The book's argument, briefly put, is that failure to disarm the Polish nationalist movement drove the Prussian-German ruling classes to embrace a radical, racially defined eastern imperialism aimed at the forcible uprooting of the Prussian Poles and the systematic Germanization of their lands. Bismarck and his successors championed an anti-Polish ‘Prussian mission in the east’ as part of their bitter struggle to shield the oligarchical and semiabsolute German Empire from its domestic opponents” (p. vii). A Poznań graduate student could not have said it better.

Fortunately, Hagen does not spend his time developing this argument but rather provides over 300 pages of interesting description of Poznań society, economics, and politics. I found much of his description of the German, Jewish, and Polish communities fascinating, as were his recitals of the area's economic and political transformations. His documentation is rich, as is his bibliography. All in

all, Hagen could well be proud of his diligent application of research techniques, and his American graduate students could rightfully use the book as a source work.

What remains to be done is original thinking. We must go beyond the role of a transmission mechanism between our areas of study and the United States. We must create. We must ask what universal meaning is imbedded in our narrow areas of study; this is the challenge to Hagen's students.

On a final note, I would take exception with Hagen's Poznań argument by asking these questions: (1) Did Germanization and an “eastern imperialism” precede German racism and, indeed, a Polish nationalist movement? (2) Was Bismarck's policy toward the Poles heavily influenced by foreign policy considerations? (3) Was the Polish issue a major factor in Prussian-German history or a side-show? If the latter, what factors in Prussian-German history determined the flow of Polish policy, and was indifference a dominant German reaction to the Poles? Hagen's recital tends to support the affirmative side of the first two questions and latter interpretation in the third, thereby contradicting the book's statement of purpose and tending toward positions I support.

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WILLIAM HARVEY MAEHL. *August Bebel: Shadow Emperor of the German Workers*. (Memoirs of the American Philosophical Society, number 138.) Philadelphia: The Society. 1980. Pp. xiv, 560, \$20.00.

In 1860 twenty-year-old journeyman August Bebel chanced into Leipzig, “still grossly ignorant of economics, political history and socialist theory” (p. 15). He had lost his father at four, a caring mother at thirteen, and he knew what it was not to eat enough. In this Saxon city workingmen were beginning to raise their heads. Bebel had the self-confidence, clarity, and simple endurance to become quickly the premier spokesman of their cause, social democracy.

The next decade he spent four years in prison, where he temporarily overcame chronically weak health. Would that we all might suffer such productive disabilities, for he now composed his classic, *Woman and Socialism*. More than normal repression was his party's lot in the eighties. Bebel now stressed *Kladderadatsch*, the notion that Second Reich institutions would collapse on their own, without such pushing and shoving from skilled workingmen as might disturb the police.

With the dismissal of Bismarck came social democracy's triumphant era. The soldier's son served



up for the voters a quite nonrevolutionary brew, containing some Marx, much utopianism, even more of liberal and welfare state demands, and a doctrinaire rejection of everything that was Prussian—with one exception. The finest service of this well-researched volume is to clear up once and for all what the great tribune would have done August 4, 1914, that day of choice between patriotic instincts and professed internationalism. William Harvey Maehl documents both Bebel's characteristic "Russia is barbaric" line and his support of the Triple Alliance; from the Reichstag committee he even knew of the projected invasion of Belgium (p. 515). Bebel died before having to approve the Prussian march on Paris. A Swiss villa had helped temper thoughts of *Kladderadatsch*, but for a man who never harmed either man or beast, his declining years were much beset by family woes.

Maehl provides party background and, irregularly, the German climate of opinion, but it is Bebel's views (although not their intellectual or other origins) that claim him most. The work is arranged in strict chronology. The inevitability that five pages after analysis of 1893 convention speeches will arrive a similar analysis for the 1894 convention is wearying; the use of odd words like "fugleman," "vaticination," and so forth, often slightly out of place, is irritating. In social democracy it was notorious that there were minorities on the left and on the right but that the majority followed Bebel. Just how did this work out? Who really led and who followed? We are not told. Then too a man who wrote one of the fundamental feminist treatises cries out for much personal analysis. It is not there. But previously there have only been short treatments, and a perceptive essay by Robert Michels. This fine volume is the first solid biography of the Second Reich's great working-class chief. A moment's consideration of Germany today suggests (p. viii) that Bebel's influence upon his native land may have been even greater than that of Bismarck.

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HANS-CHRISTOPH SCHRÖDER. *Gustav Noske und die Kolonialpolitik des Deutschen Kaiserreichs*. (Internationale Bibliothek, number 117.) Bonn: Verlag J. H. W. Dietz Nachf. 1979. Pp. 107. DM 10.

In this work Hans-Christoph Schröder of the Technische Hochschule Darmstadt has provided us with a classical German monograph—tightly circumscribed subject, intensive research, limited scope (seventy-three small pages), detailed notes, and a documentary appendix. His work is unquestionably solid and provides a useful complement to his fine

*Sozialismus und Imperialismus* (1974). Here Schröder examines the growing toleration, then sympathy and support within Germany's pre-WWI Social Democratic party for the Reich's overseas empire, a revisionist position that eventually led some Social Democrats, such as Gustav Noske, to an annexationist colonial policy during the war. More correctly, Schröder in this study focuses on the colonial ideas and politics of Noske, selecting him (quite properly) as the chief representative of that segment of the party that increasingly acquiesced in German colonial policy and ambitions. Laudably, the author is careful to differentiate between those Social Democrats like Noske, whom he characterizes as a *Realpolitiker* or pragmatist, and the theoreticians of revisionism such as Bernstein who argued for modification of the party's philosophy and practice at quite a different level of analysis.

Noske and his attitudes toward the colonial empire are couched here within the matrix of the general *Nationalisierungsprozess* evident within the prewar Socialist party. And although the Social Democrats collectively never entirely departed from a colonial posture their enemies called *reichsfeindlich*, a substantial number of them, Noske most prominently, did ultimately reach a position of approval of the colonial effort (*Bejahung deutscher Kolonialarbeit*, [p. 63]). Noske's famous 1914 work, *Kolonialpolitik und Sozialdemokratie*, two segments of which are presented as an appendix to this study, represents the culmination of that development. Schröder analyzes it both as the classic statement of Social Democratic pragmatism on the colonial question and as a sanitized version of the party's relationship to the colonial experiment since its inception in the early 1880s. His monograph is both useful and insightful, although the broader canvas of his general study provides the depth and extended analysis lacking in this short work.

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BERND F. SCHULTE. *Vor dem Kriegeausbruch 1914: Deutschland, die Türkei und der Balkan*. Düsseldorf: Droste Verlag. 1980. Pp. 183. DM 38.

Four years ago, Bernd F. Schulte published *Die deutsche Armee, 1900–1914: Zwischen Beharren und Verändern* (see *AHR*, 83 [1978]: 1,044), a bulky study of the structure, equipment, doctrines, and training of the kaiser's army in the years leading up to the First World War. Part of the material presented in that work reappears in this much shorter book.

*Vor dem Kriegeausbruch 1914*, according to Schulte, seeks to throw more light on the "decision-making process" in the upper echelons of the German government in the prewar period and, more particu-



larly, to show how "the unexpected collapse of the Turkish army in October 1912" (during the First Balkan War) affected the foreign and armaments policies of the Reich in subsequent months and years (p. 7 and following).

Schulte believes that most leaders of Wilhelminian Germany thought of the Ottoman empire as an important future partner in their conflict with the entente. The defeat of the Turks by the Balkan allies in the fall of 1912 was therefore, in his opinion, a great blow to the Germans; it forced them to reassess their situation and induced them to postpone, once again, the great showdown with France and Russia that many of them regarded as inevitable or, indeed, desirable. Schulte emphasizes, furthermore, that in the years leading up to the First World War the civilian and military authorities of the Reich worked together far more effectively than some recent studies would have us believe, and he rejects the notion that its constitutional structure made Wilhelminian Germany ungovernable.

Although Schulte has looked at an impressive range of primary sources, including some fascinating documents that survived in the regional archives of Germany, the book has some serious flaws. To begin with, it is poorly structured and contains an above-average number of ambiguous or outright confusing statements. In several parts, it is also quite repetitive in nature. Three examples must suffice.

At the beginning of his book (p. 9), Schulte suggests that the First Balkan War was a major factor in shifting German armament priorities from the naval to the army sector, yet later on we are informed (p. 37 and following) that the kaiser had in fact already announced this policy change to his commanding generals in January 1912—almost nine months before the Balkan allies pounced on the Turks.

Schulte's treatment of German preparations for aerial warfare is not very convincing, either. On page 96, for example, a memorandum by the inspector general in charge of military aviation matters is quoted so selectively that the thrust of his argument—Germany, like France, was proceeding to build up its air force in accordance with a firm plan—is almost totally obscured. Schulte, furthermore, offers a completely garbled account of what the Prussian general staff and other agencies wanted or expected in the aviation field.

Even more puzzling are Schulte's comments regarding certain Russian troop movements in the spring of 1912 (p. 45). He starts out by telling us that the move of the Russian Twenty-first Division of the Third Caucasian Corps to the Turkish border in the Alexandropol-Erevan region and the transfer of another Russian division "from Persia . . . to its garrisons north of the Caucasus" marked the begin-

ning of a redeployment of Russian forces "to the Caucasus" (which is a very dubious generalization at best). He then proceeds, rather incongruously, to tell us that "thereby" Germany's position in Europe had been strengthened, that the pan-Islamic movement and German-Ottoman cooperation were of great concern to the Triple Entente powers, "especially France," and that "this was a development that had already been examined (*überprüft*) in August 1889" by both the German Foreign Office and the general staff. To this reviewer, at least, the connection between all of these things is not clear.

Altogether, this is a book that would have greatly benefited from editorial pruning. *Vor dem Kriegsausbruch 1914* deals with an important and exciting subject and introduces some fascinating new data, including about twenty-five documents in the appendix. It is regrettable that Schulte did not present his findings in a more orderly and readable fashion.

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PETER WULF. *Hugo Stinnes: Wirtschaft und Politik, 1918–1924*. (Kieler Historische Studien, number 28.) Stuttgart: Klett-Cotta. 1979. Pp. 584. DM 78.

This is a well-researched, useful and important, but rather narrowly conceived biography of twentieth-century Germany's most dynamic and powerful entrepreneur, Hugo Stinnes. The strengths and weaknesses of the study are revealed at the outset by the author's somewhat apologetic introduction, in which he assures his readers that he is not writing in the old German biographical tradition that stressed the autonomy of the individual and that this work is in line with the contemporary trend that blends biography with the monographical treatment of a major theme, in this case, Stinnes and the problem of economics and politics. No heresy here, but also no insistence that Stinnes was one of those exceptional individuals in modern German history who managed to impose his visions, fantasies, and problems on the nation.

Peter Wulf devotes little space to the formation of Stinnes's economic empire, in many respects a forerunner of the conglomerate, arguing that it has been described elsewhere and that the sources dealing with the more complex aspects of Stinnes's business activities are difficult to find. This may be true, but it makes the emergence of Stinnes as the leader and spokesman of German industry in 1918 difficult to understand. In any case, Wulf explains it as a function of Stinnes's powerful position in heavy industry, his links to the electro-technical sector through vertical concentration, his often remarkable grasp of technical questions, and his understanding of how Germany could use its coal re-

sources and inflation to undermine France's efforts to dominate Germany and Europe after 1918 and to push forward the reconstruction of the German economy. Wulf convincingly argues that, for Stinnes, politics was the pursuit of economics by other means, and it was this single-minded conception that drove Stinnes into politics and then defined his opportunistic efforts to cope with political and social problems. It also defined the limits of his understanding and his success.

Thus Stinnes took the lead in creating the working partnership with organized labor in 1918 but then turned against labor in incomprehension and anger when labor refused to surrender the eight-hour day. He reduced the issue of socialization to questions of organization and efficiency, successfully deflecting the 1920 effort to socialize the coal industry into a discussion of his plans for vertical concentration. A year later, however, he showed much less tactical skill or political concern by demanding the privatization of the railroads as the price to be paid by the government in return for industry's placing of its credit at the government's disposal in the latter's reparations negotiations. Stinnes was the chief advocate of cooperation with French heavy industry, but he found his efforts frustrated by the unwillingness of the French government to give economics primacy over politics.

Wulf's major contribution is to provide a detailed, sound, and well-argued account of Stinnes's role in German domestic and foreign policy from 1918 to 1924, especially with respect to reparations and the Ruhr occupation. His stimulating comparison of the policies of Stinnes with those of Stresemann reveals both the narrower issue why their party, the DVP, was so torn apart and the conflict between heavy industry's narrow economic orientation and Stresemann's concern for the broader political issues. Wulf is much less successful, however, when it comes to penetrating the deeper roots of Stinnes's power or the relationship between his business interests and his politics. Stinnes had an inordinate ability to merchandise nonsense, but neither his salesmanship nor his great imagination receive their due. We are given only hints of the antagonism toward him and criticism of his views within the industrial community, but this makes his influence and power all the more puzzling. Wulf correctly notes that, as in the case of the Stinnes-Lubresac agreement, Stinnes found it quite natural to combine his personal business interests with his foreign policy activities. Yet the author does not touch upon Stinnes's desperate search for capital in the last two years of his life, and the relationship between his liquidity crisis and the policies he advocated and pursued from 1922 until his death in 1924. The posthumous collapse of Stinnes's empire was no simple function of his disappearance,

and what is missing in this study is a deeper analysis of the relationship between Stinnes's entrepreneurial style and business operations, on the one hand, and his politics, on the other.

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ROBERT P. GRATHWOL. *Stresemann and the DNVP: Reconciliation or Revenge in German Foreign Policy, 1924-1928*. Lawrence: Regents Press of Kansas. 1980. Pp. xiii, 299. \$22.00.

In a detailed and superbly researched monograph, Robert P. Grathwol has analyzed the domestic factors that conditioned the conduct of German foreign policy during the middle years of the Weimar Republic. The principal protagonists in this study are Gustav Stresemann, Germany's foreign minister from 1923 to 1929, and the German National People's party (DNVP), the largest of Germany's nonsocialist parties and an implacable opponent of Germany's postwar republican order. First in 1925 and again in 1927, Stresemann sought to create a more reliable domestic base for the conduct of his foreign policy by enticing the German Nationalists into the government. These overtures, as Grathwol convincingly argues, were not without positive effect upon the DNVP's internal development. For although the Nationalists left the government in October 1925 to protest ratification of the Locarno Pact, Grathwol maintains that by the summer of 1926 a sense of political pragmatism had developed within party circles and that party leaders had taken the first steps toward "demythologizing" their treatment of foreign policy.

Had such a trend continued, suggests Grathwol, then the DNVP might very well have developed into a moderate conservative party along the lines of the British Tories. But, as he clearly demonstrates, the new pragmatism did not extend to the grass roots of the party organization, where the leaders of the DNVP's right wing campaigned actively to return the party to the antirepublican principles that had inspired its founding. With Alfred Hugenberg's election to the party chairmanship in October 1928, all hopes that the DNVP might evolve into a conservative bastion of Germany's new republican order quickly evaporated.

By limiting his narrative to the legislative period from 1924 to 1928, however, Grathwol is unable to provide a wholly satisfactory answer to the failure of the moderates to maintain their control over the party organization. Similarly, Grathwol has eschewed any systematic analysis of the social dynamics at work within the party organization and leaves us with a picture of the DNVP that tends to be one-

dimensional and incomplete. Just as there is more to the defeat of the moderates than the intrigues of Hugenberg and his associates, there is also more to the DNVP's precipitous decline between 1924 and 1928 than its equivocation with respect to Stresemann's foreign policy.

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MICHAEL GEYER. *Aufrüstung oder Sicherheit: Die Reichswehr in der Krise der Machtpolitik, 1924–1936*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Universalgeschichte, number 91.) Wiesbaden: Franz Steiner Verlag. 1980. Pp. viii, 555. DM 96.

Michael Geyer has written much more than just another book on the Reichswehr and the Wehrmacht, since he integrates military planning and development with international and domestic politics and economics. His analysis centers on two themes: first, that in the twentieth century the conduct of war became an industrial activity, an inseparable part of a whole society's involvement in national security, and, second, that international relations had developed by the twenties on many other levels than the purely diplomatic and military, while the potential for power had shifted decisively from Central and Western Europe to the United States and the Soviet Union. As Geyer sees it, the topmost late-Weimar strategists recognized these structural changes; Groener realized that the existing German forces could not provide security and that they should be employed, if at all, only as part of a carefully coordinated and limited politicomilitary program. Later, after the depression dislocated the international economic system and after Hitler removed all budgetary and ministerial restraints, sober realism went out the window, and German military departments recklessly competed to push rearmament, creating severe dislocations in the domestic economy, provoking rival rearmament by other powers better endowed with natural resources, and placing Germany in a situation in which war was almost inescapable. Thus German rearmament was self-defeating.

Geyer's twin themes provide a powerful heuristic tool, but (as Max Weber warned) such tools are not representations of reality. Geyer's scheme, emphasizing integration in a world politicoeconomic order, implies that any crisis in that order means a discontinuity in military policy. But alternative models are possible, for example one stressing persisting organizational ties and goals; as Geyer himself notes repeatedly, the strategists always intended to return later to military power politics. In claiming that they meanwhile showed a broader vision,

Geyer strains somewhat. He telescopes indications of the continuing hope for an early war under favorable conditions (pp. 204, 216), overlooks (pp. 294–95) evidence that Groener agreed with the *Truppenamt's* militia views (see a document cited for other purposes: p. 249, n. 20, second item), and distorts Schleicher's observations on the December 1932 equality-security agreement (p. 289). Especially problematic are Geyer's efforts to argue away the distinction between civil-diplomatic and military orientations. Thus in portraying Brüning, at the April 1932 Bessinge meeting, as following a military-diplomatic policy against that of "the Bülow wing" in the Foreign Ministry (pp. 264–71), Geyer omits among other things any mention of Bernhard von Bülow's conspicuous role at, and reports on, that meeting.

Nevertheless this is an important, even path-breaking study, for it brings out the profound connection between national security and global politicoeconomic conditions. Geyer has given military history new scope.

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HORST ZIMMERMANN. *Die Schweiz und Grossdeutschland: Das Verhältnis zwischen der Eidgenossenschaft, Österreich und Deutschland, 1933–1945*. Munich: Wilhelm Fink Verlag. 1980. Pp. 795. DM 88.

This big, badly proportioned book contains rambling introductory chapters of over one hundred pages, which purport to survey the history of Switzerland, Austria, and Germany since the Middle Ages. Then four hundred pages are devoted to these countries in the years 1933–45, just as the title promises. Some overlong concluding sections examine aspects of their history since 1945. No thesis or organizational and methodological principle gives coherence to the book, or even to the individual chapters, which are full of digressions. Horst Zimmermann collects coincidences or similarities in history and repeatedly lists them as though they were evidences of something deeper than coincidence as well as of a richly informed mind. Rather than state his own view of various issues, he almost always quotes the judgment of someone else. The text is clogged with innumerable quotations from a great range of sources, more often than not on utterly trivial matters, the reliable and reputable mixed indiscriminately with their opposites as though they were equally credible. All are always fastidiously footnoted in a massive section at the end of the text. An appalling lack of rigor in evaluation of sources is often evident, for example in uncritical acceptance of the argument of Wolf Dieter Rothe that Heydrich was not at the Wannsee Conference (p. 166) or of Erich Halmdach that Russia intended to at-

tack Germany in August 1941 (p. 442). On such matters Zimmermann has deliberately quoted the eccentric views of unreliable authors rather than reputable scholarly work, which he then takes care to cite elsewhere for peripheral issues.

Despite its numerous and important deficiencies, this book has some value. The quotations, footnotes, and very long bibliography point usefully to some unpublished official documents or personal papers, as well as (especially Swiss) newspapers or journals, that historians of this period and these themes will no doubt want to examine. Birth and death dates of a great many, often rather obscure, individuals are provided, sometimes repeatedly, following mention of their names in the text. But unfortunately these dates cannot always be trusted. For example, two different sets of Ernst von Weizsäcker's dates appear at least three times, always incorrectly. Some concepts may stimulate deeper thought, if not raucous laughter. Comparative historians of fascism must now decide whether "Elektro-" and "Nuklearfaschismus" (pp. 569-70) belong with the varieties that they have already analyzed.

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WERNER MASER. *Adolf Hitler: Das Ende der Führer-Legende*. Düsseldorf: Econ Verlag. 1980. Pp. 447. DM 36.

Werner Maser is rapidly becoming a one-man Hitler publishing industry. He has written widely in popular German periodicals, and his books on Hitler and the Third Reich have gone through more than one hundred editions and translations. In this latest book he spends an inordinate amount of time putting to rest a "Führer-legend" that has been buried for years. Maser amasses great quantities of evidence, over many ponderous pages, to prove what we already know: Hitler was a terrible administrator and his government lacked any coherent political, economic, or social theory. It was a grossly inefficient jumble of competing jurisdictions further confused by conflicting *ad hoc* orders from the Führer. Franz Neumann described its essential nature many years ago (*Behemoth: The Structure and Practice of National Socialism* [1944]).

Maser also belabors the point that Hitler himself was not really the man of iron will and sure decision pictured by Nazi propaganda. The Führer was not at all sure of himself. He dithered and procrastinated and waited for circumstances to force decisions upon him. At the end of his life, he revealed the same weaknesses of leadership he had displayed throughout his entire career (pp. 15, 397). There is, of course, much to be said for all this—indeed much has already been said by writers who

have emphasized the inflexibility of Hitler's personality and his incapacity for change and development. This continuity comes as a revelation only to Maser, who had insisted in an earlier book that Hitler underwent such a miraculous personal transformation on or about the year 1942 that "within a very short time, he became quite literally a different person" (*Adolf Hitler: Legende—Mythos—Wirklichkeit* [1972], p. 337).

Maser's new revelation confronts him with a problem that he is unable to solve. If the Führer was so congenitally and chronically lacking in leadership qualities, if his much-touted leadership was in fact, as Maser repeatedly says, a "sham," a "legend," and "pure fiction," how can we account for his undeniable success in bending an entire nation to his will? To say weakly that the German people were duped, that they were not at all justified in their perception of Hitler, misses the point. The historically important and troublesome fact is not that Hitler was a vacillating and vulnerable person of confused self-image—as indeed he was—but rather that he was able to arouse mindless devotion and to make his people believe he was their infallible Führer. Since Maser is unable to distinguish between what Hitler *was* and how he was *perceived*, he is unable to cope with the central problem he has raised in his book: how could Hitler have done it—how was he able to exert such controlling power over the German people?

Maser is much better at collecting information: Hitler's service in 1919 as a member of the Communist Red Guards in Munich and his brief arrest by the Epp Free Corps; the occupations of all the early party members; the subjects of Hitler's speeches and the dozens of cities he visited in his electoral campaigns; the name of the opera singer who gave Hitler voice lessons during the 1930s; Hitler's first concern, after his victory over France, about the design of an appropriate tombstone for himself; and the physical reason why so many people found his voice inexplicably compelling—peculiar vocal cords. Listeners were unconsciously drawn to him because their brains were bombarded with sound waves of extraordinary variety and penetrative power—his voice had a remarkable tonal frequency and a range of two and one-half octaves.

One is impressed by all this *Fleißarbeit* but regrets that the author continues to indulge the annoying habits of previous volumes: he draws heavily on other historians without giving them credit; he then misrepresents their conclusions and deprecates their work, variously, as "false," "superficial," "unscientific," "absurd," and completely superseded by his own book.

The author's conclusions are curious. He tells us that Hitler failed not so much because his program was faulty as because he was not a strong enough



Führer. Actually he had some rather good ideas. If only he had been more assertive, things might have turned out very differently. He might have been able, for example, to bring about "the total ideological transformation of the civil service . . . and justice"; he could have thwarted "self-destructive and radicalizing" tendencies that, regrettably, crept into his Reich; he might have "stabilized and normalized" his government and changed it into a "traditional, conservative, authoritarian system" (pp. 399-400). Those of us who have seen Hitler as temperamentally and ideologically ill disposed to constructive social policies or traditional political forms and have detected irrational impulses and destructive tendencies in him are, Maser insists, completely mistaken. Hitler was basically a normal, well-meaning but ineffective person. His positive plans were frustrated by circumstances beyond his control. "The essential fact," we are reminded, is that for the entire period after 1939 Hitler, somehow, "found himself at war" (p. 400).

Although the last pages of the book certainly justify the claim that Maser's picture of the Führer "has never been seen before," we may hope that his publisher is mistaken in asserting that this book sets a "new standard" in Hitler studies.

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RAFFAELLA GHERARDI. *Potere e costituzione a Vienna fra Sei e Settecento: Il "buon ordine" di Luigi Ferdinando Marsili*. (Annali dell'Istituto Storico Italo Germanico, number 2.) Bologna: Il Mulino. 1980. Pp. 516. L. 20,000.

One of the aspects of early modern Europe most neglected by historians in this country is the close interrelationship and interpenetration between the Italian- and German-speaking lands, Austria in particular. In Europe, however, considerable interest in this problem has been generated in recent years. There is even a special *Istituto storico italo germanico* appropriately located in Trent. This is the second work to be published under its auspices.

Raffaella Gherardi's book focuses on the work of Count Luigi Ferdinando Marsili, a Bolognese scientist, soldier, and economic planner who entered the service of the imperial army in 1682 and remained in the emperor's pay until he was deprived of his rank and property by court martial following the fall of the fortress of Breisach to France in 1704. Most of his time was spent in the campaigns against the Ottoman Turks, and during the Peace of Karlowitz in 1699 he worked as special commissioner for settling the frontiers. The author has been researching the massive collection of his papers at the University of Bologna library for a long time. But,

as the title of the book indicates, she has broadened the framework of this monograph to make it a study in some depth of the economic and financial ideas and policies that prevailed in Vienna from about 1684 to 1699, that is, during the period between the great age of mercantilist cameralism associated with the names of Becher, Hörnigk, and Schröder and the later period when the Dutch controlled the Austrian state economy. Marsili was active at that time as an adviser and observer.

These years were marked by the process of molding the disparate collection of Habsburg hereditary lands into a centralized monarchy equipped with a more rational system of administration. It was thought that Hungary would make a good starting and proving ground for the experiment since it was newly reconquered from the Turks. The Viennese government imagined it could easily proceed by right of conquest to override all local opposition and set up its authority. Hungary could also be turned into an economic asset. In this connection Marsili becomes of importance. A mercantilist, he advocated a policy that would make Hungary the great transit market with the Ottoman empire, not only for the Balkans but also for the whole of the eastern trade with northern Europe, and so bypass English and Dutch shipping. At court Marsili enjoyed the protection of such influential figures as Strattmann, Kinsky, and Kaunitz. One interesting feature of Gherardi's work is the way she traces the various political currents that prevailed at court—the Habsburg court party of Strattmann, the Bohemian estates party led by Kinsky, and the military party headed by Montecuccoli—and their interaction, especially in the face of the Hungarian problem. Her conclusions on this issue do not coincide with those of Ingemar Bog's work on *Reichsmercantilismus* (1959).

Yet the work is flawed. This study, which concentrates so much interest on Hungary, makes no use of Hungarian sources, except a very few in Western languages. The author also tends to lean too heavily on the work of Jean Béranger. It is always a temptation to mine a French *thèse d'état*. The pity is that the work was written before the author had access to R. J. W. Evans's *The Making of the Habsburg Monarchy, 1550-1700*. But there is something even more seriously wrong with the overall organization. A gifted playwright may hold the interest of an audience with a drama divided into two parts of five acts each, every act having three to five scenes; the author of this learned work only confuses her readers. Long quotations on almost every page, even if they are relevant, impede the flow of the arguments. Finally, the work finishes with the second chapter of the second part. The last three chapters, although of intrinsic interest (discussing such issues



as the influence of Hermann Conring's teaching on practical statecraft on Marsili by way of Leibniz) are irrelevant to the thesis.

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DAVID S. LUFT. *Robert Musil and the Crisis of European Culture, 1880-1942*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xii, 323. \$18.95.

In view of the range of Robert Musil's work it is fitting that David S. Luft comprehends his oeuvre within the orbit of European culture in general rather than in terms of literary history. Musil, the author of *The Man without Qualities*—one of the foremost, and unread, great novels of our times—was both a thinker and a poet, in that order. In fact he was also a trained scientist and a perspicacious philosopher.

Luft brings this out clearly in this study, based on a rich background of secondary and primary sources, the latter chiefly taken from Musil's *Nachlass*. Notwithstanding the fame Musil acquired after his untimely death in exile from Hitler's Reich, he will always appeal only to a select few. In this respect Musil compares with his Austrian contemporaries, Hermann Broch and Heimito von Doderer. They were just as atypical as Musil of Austrian writers. Luft correctly states that Musil "was an Austrian and a poet, but he did not think of himself as a Austrian poet within Austrian literature" (p. 294). Whether Musil's importance as a writer is anchored in his recognition of the breakdown of liberal philosophy is more debatable. According to Luft, "he explored the assumptions, limits and collapse of liberal values in the generation of 1905" (p. 294). Yet, who was this generation of 1905? According to Stuart Hughes's interesting concept, its artists became creative during the decade from the first Moroccan crisis of 1905 to the outbreak of the First World War. Musil saw himself, however, as part of a far broader group that reached maturity between 1870 and 1890. Within this group liberalism did not stand by any means for the unqualified notion of intellectual and political decadence.

Here, as in many other places, the author pushes individualism into the background and perceives almost everything as representative of something larger, often in the form of antinomy. Especially troublesome in this respect is the trend toward categorization. Why for instance do Musil's parents express "the tensions in liberal culture between discipline and eros, state service and individualism" (p. 26)? Why did not just Musil but his whole generation receive "the Kantian doctrine and romantic

aesthetics via Schopenhauer and Mallarmé" (p. 39)? Would that be true also for such intellectual antipodes as Oswald Spengler and Bertrand Russell, who both belonged, according to Musil's estimate, to his own generation? Why above all is Fritz Ringer's thoughtful concept of "mandarin" and "mandarinate" so overworked? Intellectuals appear as mandarins throughout the study, their social status is that of a mandarinate that can be clearly distinguished from other social classes and ideologies.

Such method may distort and indeed frequently has distorted studies in the realm of intellectual history. Thanks to Luft's impressive command of his subject matter, his rich cultural background, and the artistic sensitivity visible throughout the book, this technique has done only minor damage to the work. In substance it is not only serious and competent but also a highly impressive product of intellectual effort and scholarship that does great credit to the author.

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GUIDO RUGGIERO. *Violence in Early Renaissance Venice*. (Crime, Law, and Deviance Series.) New Brunswick: Rutgers University Press. 1980. Pp. xv, 235. \$18.50.

The study of violence and related forms of social deviance in early modern Europe has intensified during the last decade. It is a rich subject, both for its contemporary resonances and in terms of the surviving archival materials. New data has been appearing, drawn from such centers as Seville, London, Dijon, Florence, and Rome. Although scholars like Ruth Pike, Barbara Hanawalt, John Langbein, and others have much to offer concerning the form and frequency of criminal violence and the efforts of municipal and national governments to limit and punish malefactors, the sources and their modern commentators provide far less instruction on the mentalities of violent men and women in the Renaissance.

Guido Ruggiero's *Violence in Early Renaissance Venice* is no exception. Ruggiero has sifted many of the extensive court records for Venice from 1290 to 1406, and surveyed the chronicles as well. He is interested both in criminal violence and in the ways the Venetian authorities perceived and responded to such behavior. His approach is sociological, especially in the last two of his three sections. Part 1, "The State and Violence," considers the Venetian system of social control, with particular emphasis on institutional structures such as magistracies and police, and on the legal system, the criminal code,

and the way it appears to have been applied. Part 2, "The Social World of Violence," seeks to break down recorded criminality in terms of class. Ruggiero uses five categories—nobility, clergy, people of importance, laborers, and marginal people—and offers a statistical profile of violent crime in each. He concludes that workers seem "less violent" than nobles, but more so than "important people." He sees the function of the latter group as mediating between the nobility, distinguished by its "arrogant violence" and "schizophrenic combination of bourgeois commitment to order and neofeudal commitment to prestige and power," and the lower orders, whose violence was "more spontaneous and reactive" (p. 120).

Part 3, "The Perception of Violence," considers the evidence on how the Venetian nobility viewed the four most serious crimes, which Ruggiero calls crimes of speech, assault, rape, and murder. Although this section tends to restate a good deal of part 1, it does so from a different perspective. This section is more inferential, and less dependent on statistical demonstrations. One senses the lessening of rigor in sentences like "The rape of nuns was a popular diversion, especially for the nobility" (p. 169). The instances cited, rather than supporting a judgment that might well be overstated, would seem to suggest that the term "rape" may cover a much wider variety of sexual manifestations in the Renaissance than in common usage today.

Although this book diligently presents a substantial amount of interesting new material and some extraordinary case histories, it has two major defects. The first is that the interpretations are not always adequately supported by the evidence and even seem at times to flow from prior assumptions about the nature and behavior of socioeconomic groups. This is particularly evident in the treatment of the Venetian patriciate, where the careful formulations of other scholars (such as Stanley Chojnacki) tend to be dismissed in cavalier fashion. The second is that, although Ruggiero seeks "to encourage similar studies in other cities," the book suffers from the absence of a comparative dimension and does not seem anchored in the recent literature on its subject. In the end, one wonders what is distinctive or unique about Venetian criminal law, its applications, and the phenomena it sought to regulate.

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FELIX GILBERT. *The Pope, His Banker, and Venice*. Cambridge: Harvard University Press. 1980. Pp. vi, 157. \$12.50.

In February of 1511, Agostino Chigi, an immensely wealthy Roman banker, came to Venice for a stay that would last through the summer. He spent a

great deal of time in secret negotiations with government officials, and it finally transpired that he was negotiating a loan of money badly needed by Venice for the costs of the war of the League of Cambrai. In the end, Chigi agreed to lend a huge sum in return for a complex arrangement guaranteeing that he could sell alum from the Papal States to Venice, or in the Venetian marketplace, at a good price. In the following years disagreement over repayment arose, and final settlement took place only after Chigi died in 1520.

Felix Gilbert rather tentatively suggests, especially near the end of his brief narrative of these events, that they illustrate aspects of the larger story of the decline of Italy in the early sixteenth century. The Venetian response to decline, he says, was to dwell on images of past greatness. Pope Julius II, on the other hand, swam against the historical current and sought not only to recover lost lands but also to build up the economic might of the papacy. As part of this policy he and Chigi sought to push the sale of Papal States alum. But the story of the loan seems to me to be only marginally related at best to these reactions. If anything, it illustrates Venice's concern not with a mythicized past but with survival. Furthermore, Julius II was only tangential to the arrangement of the loan and died before the long struggle over repayment began.

Gilbert's contribution lies not so much in the suggested broader interpretation as in the narrative itself, which he has written in an almost novelistic fashion, and in his learned and lucid explanations of Venetian decision making, the roles of court bankers, the economic policies of Julius II, and the purposes of Renaissance banquets and ceiling paintings.

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ANTONIO CONFALONIERI. *Banca e industria in Italia, 1894-1906*. Volume 2, *Il sistema bancario tra due crisi*. (Universale Paperbacks il Mulino, number 94.) Reprint. Bologna: Il Mulino. 1980. Pp. 516. L. 8,500.

This is the second volume of a three-part study of the link between Italian banking and heavy industry during the years of Italy's initial large-scale economic growth. Based on a wide range of sources, including the archives of the Banca Commerciale, the Credito Italiano, and the Banca d'Italia, minutes of the meetings of the governing boards of those banks and others, and the private correspondence of such prime movers as Otto Joel and Federico Weil, Antonio Confalonieri's work is a compendious and authoritative treatment of the role played by the great banks in the capitalist development of the peninsula. In addition to its many virtues, however, this

volume is subject to shortcomings often associated with such ambitious and exhaustive accounts.

Beginning with the calamitous fall in 1893 and 1894 of the Credito Mobiliare and the Banca Generale, Confalonieri traces attempts to reconstitute Italian banking on a more secure footing, attempts that eventuated in the massive influx of foreign (mostly German) capital and expertise. The upshot of such efforts was the formation of the two great "mixed" banks, the Banca Commerciale and the Credito Italiano. Confalonieri treats the inauguration of these two institutions in great detail, transcribing the rather tortuous negotiations of Joel Weil, and their German-Swiss counterparts in the case of the Banca Commerciale and the equally difficult (but less abundantly documented) arrangements between the Banca di Genova (forerunner of the Credito Italiano) and its own trans-Alpine investors. In a succeeding chapter Confalonieri traces the difficult financial and economic course that Italy traversed between the early 1890s and the first years of the twentieth century.

One lengthy chapter deals with the founding and evolution of the Banca d'Italia. From its difficult inception in 1893-94, the new bank was intended as the chief institution of currency issue, and Confalonieri presents a wealth of material on the policies of the state bank and its relations with the Ministry of Finance and with "ordinary" banking houses. Besides functioning as a bank of issue and as the repository of bullion reserves, the Banca d'Italia also operated as a kind of commercial bank, serving to liquidate the failed Credito Mobiliare and Banca Generale and to provide both capital and leadership to fledgling Italian industries (especially in the metals sector). Confalonieri thus implies that the boom of the early twentieth century originated nearly as much in state action (through the Banca d'Italia) as in the operations of the great "mixed" private banks and their foreign investors.

In completing this volume, Confalonieri turns back to consideration of the great "mixed" banks, and specifically to discussion of the Credito Italiano and the initial phase of its development. Considering first the efforts of the new bank to assimilate and organize the investments of its predecessor, the Banca di Genova, Confalonieri discusses the development of policies that allowed the Credito Italiano to act as an industrial entrepreneur as well as a source of financial credit. He reviews the central role played within the bank by G. B. Pirelli and the relationship between the bank and Pirelli's own firm (p. 329), and he provides a case study of the linkage between the Credito Italiano and the Neapolitan shipbuilding firm of Pattison (p. 364). Owing perhaps to the prevailing scarcity of documentation, Confalonieri unfortunately provides an all-too-brief overview of the interests of the Credito

Italiano in the steel and metallurgical sectors—clarification of which would have greatly aided our comprehension of Italy's early twentieth-century boom and its origins. One hopes that in his forthcoming volume on the affairs of the Banca Commerciale the author will provide more complete coverage of the connection between the great banks and these industrial firms.

It is to be hoped as well that Confalonieri will provide further analysis of the linkage between banking and the larger economic, social, and political history of Italy. Although he makes specific reference to this task (p. 289), Confalonieri seems mainly to content himself in the present volume with a minute discussion of banking institutions to the detriment of the wider historical context. Despite this weakness (and the related problem of rather arbitrarily terminating his investigation with the slowing of the boom in 1906), Confalonieri achieves much in his study, and the present volume will no doubt serve as a fundamental source for future consideration of Italian finance and its industrial connections.

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ALEX N. DRAGNICH. *The Development of Parliamentary Government in Serbia*. (East European Monographs, number 44.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. 138. \$10.00.

A Serbian prime minister, approached in London at the end of the nineteenth century with a question about the level of industrial development in his country, answered, "At home we call matches a machine." When the Austrian Civil Code was adopted in Serbia in 1844, the country had among its presiding justices only one with a law degree. Yet, some forty years later, in 1886, the Academy of Sciences was founded, with scholars of European-wide reputation. The small Serbian state, born in the fire of a peasant revolution, was engaged in a life-or-death struggle with the surrounding Habsburg and Ottoman empires and finally came out as a victor in 1918. This story of changes and modernization has intrigued both historians and political scientists.

Alex N. Dragnich presents his study of parliamentarianism in Serbia within a general historical background. He divides the subject into three parts: part 1 deals with the task of nation building and covers the period between 1804 and 1868. Part 2 traces the struggle for parliamentary government during the decades from 1868 to 1903. Finally, part 3 discusses parliamentary government in operation, 1903-18. The author includes a chapter on the role of the military in political development.

Born in the United States, Dragnich maintains a warm feeling of sympathy for the land from which his ancestors came. He is primarily a political scientist and deals with Serbian institutions in their political, not social, context. Serbian political institutions developed in the midst of a conflict between traditionalism and modernism. Modernization meant transplantation of European institutions onto a Balkan body that was both receptive and recalcitrant at the same time. The centralizing tendencies of state authorities were met by the decentralizing tendencies of local leaders supported by village localism. A peasant revolution resulted in a military monarchy. The real polarization between conservatives and liberals occurred only from the 1870s on.

Dragnich's study summarizes what we already know. It does it in a clear, well-written, and organized way. For the English-speaking public it is an informative study that offers all sorts of comparisons with the contemporary world. The book will be useful for university students and for a wider public interested in the area.

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ZBIGNIEW MAZUR. *Pakt Czterech* [The Four-Power Pact]. Summary in English. (Studium Niemcoznawcze Instytutu Zachodniego, number 33.) Poznań: Instytut Zachodni. 1979. Pp. 315. 75 Zł.

Zbigniew Mazur addresses two broad questions in his study of the Four-Power Pact: (1) could it, in the circumstances of the time, serve to maintain the balance of power and European security—that is, was it a real alternative to the collective security system embodied in the League of Nations? and (2) what were its side-effects on France's relations with its East European allies? The book brings Polish readers the first objective and extensively documented study of the topic and will, therefore, be most welcome to them. Except for Polish archival materials, however, the author does not bring to light documentation that is new to the Western reader. He is understandably limited by his lack of access to British, French, and Italian archives, as well as to the national press of those countries. We can assume that Polish currency as well as political restrictions explain his reliance on published sources for Western nations. So far as the Little Entente is concerned, the Czechoslovak, Rumanian, and Yugoslav diplomatic archives are still generally inaccessible.

Mazur gives negative answers to the two questions posed at the beginning of his study. He disagrees with the view of Konrad Jarausch, Jean-Baptiste Duroselle, André Chastenet, and others

that the Four-Power Pact could have safeguarded the European balance of power and thus provided peace and security for Europe. He not only believes that the depression and the concurrent rise of Hitler to power made this unlikely, but he also emphasizes that security for Western Europe could not survive without the provision of security for Eastern Europe as well. This was, of course, the view of Polish statesmen at the time, and it is hard to deny that history proved them right. This view, however, was alien to Western statesmen, as it still is to some historians writing today.

The author documents the weakening of Polish reliance on France and the divergent attitudes of Poland and Czechoslovakia toward the Four-Power Pact. He argues, nevertheless, that the basic opposition to frontier revision, as expressed by Poland and the Little Entente states, was decisive in the French dilution of the pact in its final form. His view here could only be confirmed or refuted by a study of the French press. A cursory acquaintance with it suggests that it was sufficiently distrustful of Germany to exert a significant influence on the government. He is correct in pointing out that Mussolini's chief motive in suggesting the pact was the desire for closer Franco-Italian relations. This is documented by Edward R. Bennett in his recent book, *German Rearmament and the West, 1932–1933* (1979).

This is a detailed diplomatic history of the origins and negotiations of the Four-Power Pact with useful information about its negative effects on France's allies in Eastern Europe. A definitive study, however, must be based on work in British, French, Italian, as well as Czechoslovak, Rumanian, and Yugoslav diplomatic archives and on the contemporary press of Great Britain and France.

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HANS-ULRICH WEHLER. *Nationalitätenpolitik in Jugoslawien: Die deutsche Minderheit, 1918–1978*. (Sammlung Vandenhoeck.) Göttingen: Vandenhoeck und Ruprecht. 1980. Pp. 164. DM 29.

This brief monograph is more remarkable for its tone than its content. Despite some lapses, Hans-Ulrich Wehler has examined a delicate topic *sine ira et studio*. *Nationalitätenpolitik in Jugoslawien* is missing both the suspect lacrimoniousness of earlier West German efforts and the self-serving, equally suspect indignation of its East German brethren. Wehler excoriates neither the Yugoslav *Volksdeutsche* nor their hosts for the events of the 1940s; both sides are made to bear their share of the blame for the tragic end of an ethnic symbiosis. There is more than enough to share.

The work is divided into three parts: the twenty interwar years, the war itself, and the postwar years



through 1948. Despite the subtitle, the book closes with the practical elimination of the German minority in Tito's Yugoslavia in the latter year. Since then, their numbers—once over half a million—have declined to a point where they have no bearing on the consciousness of their Slavic co-citizens.

The first section is the weakest in its failure to elucidate the peculiar historical burdens that afflicted Slavic-German relations in the newly born Kingdom of the Serbs, Croats, and Slovenes. Without this context, the reader can understand neither the internal tensions within the German minority nor the ease with which the pro-Hitler element took leadership during the 1930s. This difficulty is compounded by the sparseness of Wehler's remarks in part 2 on the aid given the invaders by the Yugoslavian Germans in 1941 and their active participation in the government of occupation.

Much of the second part is occupied with detailing the administrative relations between the Reich agencies and the German civil population. The impression is given that there was considerable resentment toward Wehrmacht plans to conscript German men, a construction that would surprise most Yugoslavs of my acquaintance. The interaction between the now officially favored minority and the mass of South Slavs during the war years is hardly mentioned.

These sins of omission are counterbalanced in the final section, which deals with the treatment of the helpless Germans who stayed or were left behind by the retreating Nazi army. In place of the earlier blanket condemnations of the indisputable "excesses" committed by the Tito forces in 1945–46, there is a measured and factual explanation of the new regime's attitude and policy. Mention is made of the extreme hardships that accompanied the expulsion of the remaining German-speaking population from the Yugoslav lands (more than half of the 1941 *Volksdeutsche* had left or been resettled by 1945), but there are no gratuitous appeals for sympathy, and the whole painful episode is handled with model reserve and factualness.

Wehler is admirably equipped to work in the sources available to him. He has used Serbo-Croat as well as German documentation for the prewar years, but he has necessarily depended on Nazi files for the war era and on West German archives for the years thereafter. The text is accompanied by a variety of useful statistical data.

It is a stunning indication of the changes in focus of the historiography of Southeast Europe that this little work is until now the only one dealing expressly with the termination of the Germanic presence in Yugoslavia, a presence that goes back seven centuries and that has attracted a whole corps of historians to its origins and extension.

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RANDOLPH L. BRAHAM. *The Politics of Genocide: The Holocaust in Hungary*. In two volumes. New York: Columbia University Press. 1981. Pp. xlii, 594; 596–1,269. \$60.00 the set.

Just before and during World War II, Hungary re-acquired from Czechoslovakia, Rumania, and Yugoslavia a large part of its pre-1918 territories. As a result of these re-acquisitions, its population rose to over 14,600,000, of which approximately 725,000, or not quite 5 percent, stated that they belonged to the Jewish religion. Since the antisemitic laws, adopted in the same period, branded more than 100,000 Christians as racial Jews, and since during the war a substantial number of Jews, especially from Slovakia, took refuge in Hungary, the total number of people affected by Hitler's genocide program hovered around 900,000. The Hungarian section of the "Final Solution" was horrifyingly successful: about two-thirds of the Jews and "Christian Jews" did not survive the war.

The story of the Hungarian holocaust is in some ways more tragic than that of the others because almost all of the killings took place in the last year of the war. A large part of these killings were accomplished early in the summer of 1944 at Auschwitz, within the space of a few weeks, and the chances of mitigating the scope of the massacre were excellent.

This admirable monograph by Randolph L. Braham, the world's foremost expert on contemporary Hungarian Jewry, argues convincingly that a little more resolution and foresight on the part of the Jews and their protectors, or perhaps a little more luck, could have saved hundreds of thousands of lives. If Prime Minister Miklós Kállay had not prepared so openly and so fruitlessly, in 1943 and early 1944, for satellite Hungary's surrender to the Allies, thereby provoking the German military intervention in Hungary on March 19, 1944; if, following the German invasion, the regent, Admiral Miklós Horthy, had not stayed in office, thereby sanctioning the deportation of almost half a million Jews in one of the smoothest large-scale administrative operations in history; if the leaders of Hungarian Jewry had not systematically withheld the truth about the Holocaust from their co-religionists; if the Hungarian Jews had been less law-abiding; if Horthy, once he stayed in office, would have awakened from his stupor earlier than July 7, 1944, when, by issuing a simple order, he forbade the deportation of some 200,000 Budapest Jews (the Jews of the countryside were gone by then), and thereby caused Eichmann to withdraw in fury from Hungary; if the Allies had been willing to bomb the railway line leading to Auschwitz—clearly the situation would have been more favorable for the Jews.

The list of missed opportunities and errors is long, and they are enumerated in the book relentlessly. The monstrosity of the German and Hungarian



Jew-killers is crystal clear, but almost equally clear is the historical responsibility of those whom Braham calls the "onlookers": the Allies and the neutral powers, the Vatican and the International Red Cross, the Hungarian Christian population and the church authorities, and the world leaders of Jewry. The conclusions are not new, yet they bear repetition: it was possible to say "no" to the Nazis, no harm came to the few Hungarian officials who refused to participate in the genocide, publicizing the Nazi atrocities was more efficient than bribing the SS or discreetly petitioning the Hungarian authorities, and, finally, it was far less risky for a Jew to go underground than to obey orders.

In the brief but comprehensive preface to his long book, Braham points to the many paradoxes in Hungarian Jewish history—all of which might have contributed to the final tragedy. Enormously successful, highly patriotic, and assimilationist in its elite, Hungarian Jewry before 1914 became identified in the eyes of the Christian public with capitalism, liberalism, urbanism, modernism, and even with Magyar jingoism. In 1919, however, because Béla Kun's soviet republic was led almost entirely by rebellious Jewish intellectuals, Hungarian Jewry also became identified with Bolshevism, anarchy, and internationalism. A year later, Admiral Horthy's counter revolutionary regime adopted the first anti-Jewish law in post-World War I Europe, but successive governments tended to ignore the law, and the Jews, especially in business and the professions, were allowed to thrive again.

Beginning in 1938, a series of increasingly severe antisemitic laws was put into effect, further impoverishing the poor, and between August 1941 and January 1943 a total of 63,000 Jews were killed, including 42,000 labor servicemen on the Russian front, 20,000 "alien" Jews deported to the Ukraine and handed over to the SS, and 1,000 alleged Yugoslav partisans in the Voivodina. Yet, throughout those years, the Horthy regime, especially Prime Minister Kállay and Minister of the Interior Ferenc Keresztes-Fischer (one of the "righteous"), courageously protected the lives and property of over 800,000 Jews from incessant German entreaties. In March 1944, following the German invasion, Horthy allowed Hungarian authorities to collect and deliver to Eichmann and his 200-odd transportation specialists all the Jews of the countryside who were not in labor service, but in July of the same year Horthy prevented the deportation of the Budapest Jews. In all of this, Horthy's contempt for the "Eastern" Jews and his sympathy for the assimilated Budapest Jews played an important role, but, actually, the true Hungarian Jew-killers were less particular, and they treated the rich and the assimilated worse, if possible, than the poor Jews from the east.

Following Horthy's forced resignation in October 1944, the killings and deportations began anew under the fascist Arrow Cross regime but this time practically without any assistance from the Christian population. As the SS and the Gestapo complained repeatedly, they received in the spring of 1944 more denunciations in Hungary than anywhere else, but in the fall and winter of 1944 Christians hid tens of thousands of Jews. The Hungarian officer corps was notoriously pro-Nazi and antisemitic, yet it was the army that enabled tens of thousands of labor servicemen to avoid or to delay deportation and thus to survive the war. The Hungarian conservative aristocrats, all counter revolutionaries and mostly antisemitic, proved to be the Jews' best friends during the war. The Hungarian neolog and orthodox leaders in the Jewish Council were too patriotic and law-abiding to call for passive resistance or even publicly to protest the deportations, but individual members of the Council risked their lives on behalf of other Jews. As for the Zionists, among them the notorious Rezső Kasztner, they did indeed collaborate with the SS—for which Kasztner was murdered later in Israel—but they also saved thousands of Jewish lives.

All this is told with a remarkable mixture of human sympathy and academic detachment, with impeccable documentation, and with a bit too many details for the nonspecialist. One can disagree, for instance, with the author's bad opinion of Prime Minister Kállay (1942–44), but the book's only fault lies rather in the many repetitions. Because each chapter stands on its own, reading the 32 chapters consecutively can be slightly disconcerting. All in all, however, this is a crucial book for the understanding of modern Hungarian, Jewish, and German history.

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ROBERT R. KING. *A History of the Romanian Communist Party*. (Histories of Ruling Communist Parties, Hoover Press Publication, number 233.) Stanford: Hoover Institution Press. 1980. Pp. xvi, 190. \$8.95.

*A History of the Romanian Communist Party* is a concise study well suited for the series on the histories of the ruling Communist parties edited by Richard Staar and published by the Hoover Institution. It is brief, readable, and informative. It is not, however, a history of the Rumanian Communist party in the strict sense of the word.

The data available for the writing of a systematic history of the party could be pieced together by careful and extensive research in Rumania, the Soviet Union, Israel, France, Spain, and a few other countries where knowledgeable persons and docu-

mentary sources are available. Such an effort would have been both uncalled for and unrealizable in this instance. Consequently, Robert R. King has produced an intelligent recapitulation of current interpretations of the known history of the Rumanian Communist movement interspersed with interesting observations of his own, all within a factually oriented framework.

The red thread, apparent throughout the book, is the use of history by the Rumanian Communist party for legitimizing its power and policies. King rightly notes the difficulties encountered by the party in claiming any historical legacy given the Rumanian Communists' insignificance in the history of Rumania and in the international Communist movement before the end of World War II. Nevertheless, he tends to accept certain premises advanced by Rumanian sources and by foreign writers on Rumanian Communism that provide a framework for a history of the party but that are not always demonstrably accurate. It would be difficult to agree with the view that the national minorities were predisposed to Communism in the thirties or to assume that the use of history by the present leadership is related to anything more than the retention of power. In other instances, inferences are inconclusive. Although the author rightly observes that the right-wing Christian populist movement identified with the Iron Guard in the thirties had many followers among the working class, he fails to make the connection between authoritarianism of the right and of the left in the ultimate synthesis of the two—and of the entire Rumanian autocratic tradition, for that matter—in the national socialist Rumanian Communist state.

It is, however, fair to say that within the limits imposed on him by the scope of the series and by the lack of data the author had to focus his study on Communist policies and practices since World War II. Therefore, the largest part of the volume is devoted to an account of actions by the Rumanian Communist party rather than to an appraisal of the relationship of the party and of its leaders to the historic evolution of Communism, whether national or international, or to the history of social and political unrest in Rumania proper. King's account of Communism in Rumania updates such works as Ghita Ionescu's *Communism in Rumania, 1944-1964*, but it does not provide us with a comprehensive history of the Rumanian Communist party. That remains to be written.

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ZDENĚK L. SUDA. *Zealots and Rebels: A History of the Communist Party of Czechoslovakia*. (Histories of Ruling

Communist Parties, Hoover Press Publication, number 234.) Stanford: Hoover Institution Press. 1980. Pp. xi, 412. \$8.95.

This is the eighth volume in a Hoover Institution series dealing with the origins and development of ruling Communist parties. The author of this long book on the history of the Communist party of Czechoslovakia, Zdeněk L. Suda, is an associate professor of sociology at the University of Pittsburgh. The best portions of the book are sociological analyses of the Czechoslovak electorate and descriptions of the personnel changes in the leadership of the Communist party of Czechoslovakia. Its main weaknesses are Suda's lack of knowledge of history, political theory, and sources.

Although the author states that "a complete list of background literature is included in this volume" (p. xi), nowhere does one find any reference to archival materials or to the many easily accessible volumes of published documents. There is also no mention of Gordon Skilling's articles on the formation and the early development of the Communist party of Czechoslovakia published in the *Slavic Review* (October 1955 and April 1960), Paul Zinner's *Communist Strategy and Tactics in Czechoslovakia, 1918-1948* (1963), or this reviewer's *Czechoslovakia's Role in Soviet Strategy* (1978).

The space limitation does not allow listing of the large number of factual errors, oversimplifications, and distortions. A few examples must suffice. Suda writes that the Czechoslovak National Council in Paris, "directed by Professor Thomas Garrigue Masaryk and representing all shades of currents of Czech and Slovak political life," had its office in Russia "since the outbreak of the war [WWI], successfully coordinating the efforts of the various groups engaged in the campaign for Czechoslovak independence" (p. 6). In fact, the Czech National Council was established in Paris by an agreement between two deputies of the Vienna parliament, Professor Masaryk and Josef Dürich in 1916, and the Czecho-Slovak campaign and military force in Russia was organized by Czecho-Slovak associations in Russia and the tsarist military, not by the nonexistent National Council.

Suda does not realize that the Czech National Committee that proclaimed the Czechoslovak Republic in October 1918 (p. 20) was headed by Dr. Karel Kramář, while the Czech National Committee "constituted in the fall of 1916," that, according to him, "had played a key role in the campaign for Czechoslovakia's independence" (p. 19), actually professed its loyalty to Austria-Hungary and the "ancient, glorious, Habsburg dynasty."

Suda's description of Masaryk as "one of the statesmen most hated by the Bolsheviks because of . . . his role in the Siberian anabasis of the Czech-

oslovak Legion during the Russian civil war" (p. 103) cannot be squared with Masaryk's agreement with the Bolsheviks on the transfer of the Legion to France and recommendation for their recognition as the *de facto* government of Russia in his memorandum for President Woodrow Wilson in April 1918. The Legion revolted against the Bolsheviks and the policy of neutrality decreed by Masaryk; the Legion's belligerency, however, became the basis for the Allied recognition of the Czecho-Slovak National Council.

Turning to the period of the Second World War, it should be noted that in 1944 not even the British called the Czechoslovak government "provisional" (pp. 178, 180). The four Communists Suda mentions as members of the Czechoslovak National Committee in Great Britain, including the Sudeten-German Communist, were not in the National Committee (pp. 166-67). Therefore, the assertion that "The USSR recognized the National Committee as a government before a Communist delegation was seated in it" is nonsense. Even when Beneš suggested it in December 1943, the Czech Communist leaders refused to seat any Communist in the London (Beneš) government for strategic and tactical reasons, which have been explained in this reviewer's book, mentioned above.

Suda's discussions of Czechoslovak-Soviet relations are oversimplified to the point of distortion. Despite the evidence to the contrary, Suda claims that the Czechoslovak "Communists were prepared to observe the rules of the democratic game in December 1943" (p. 173). Relevant sources show conclusively that Beneš made far-reaching commitments to both Stalin and the Czechoslovak Communists in Moscow in December 1943. He promised to coordinate his foreign policy with the Soviet Union, and he assured the Czechoslovak Communist leaders that their party would become the leading political force in restored Czechoslovakia. Suda's lack of understanding of Marxism-Leninism makes his analyses of the developments within the Communist movement superficial and of questionable validity. The national-democratic revolution (1945) gradually developed into the socialist revolution (1948), but Suda fails to notice the overall Communist strategies and the changing tactics before and after the events.

The author clearly does not understand the significance of the events in Slovakia in the fall of 1947. His assertion that in February 1948 Beneš was not kept informed by the resigning ministers of the National Front has been refuted by those individuals and by others. Had Suda read the records of meetings between Beneš and the Soviet leaders and the Czechoslovak Communists in Moscow in December 1943, he would have had no difficulty in understanding the February events for which the appropriate name is "putsch" (p. 222).

The claim that "Within twenty-four hours [of the Slovak uprising] the whole of Slovakia was in the hands of the rebels, with the exception of the capital city" (p. 175) betrays Suda's lack of knowledge of the basic facts in the Slovak uprising in Banská Bystrica and his lack of understanding of the complexity of the preceding and following events. The same applies to his analysis of the Ruthenian situation (p. 177).

Although Suda devotes two chapters to the Prague Spring and its aftermath, he makes no reference to the book by Zdeněk Mlynář, a former member of the party Presidium and secretary of the Central Committee, *Mráz přichází z Kremle* (1979). The book has appeared in English as *Nightfrost in Prague* (1980). Mlynář has come much closer to understanding the real reasons for the Soviet intervention—their strategic and military interests in Czechoslovakia—than has Suda.

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FRANK W. THACKERAY. *Antecedents of Revolution: Alexander I and the Polish Kingdom, 1815-1825*. (East European Monographs, number 67.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1980. Pp. 197. \$15.00.

This is a study of intentions and policies of Alexander I relating to the Congress Kingdom of Poland and the Russian empire prior to the revolution of 1825 and the 1830 Polish uprising. Frank W. Thackeray's interest and findings are centered around selected issues. One is Alexander's determination to keep the kingdom under his authority and Russian domination. The tsar's broader policy goals included a hope that the unification of the Lithuanian lands with the Polish kingdom would help him bring constitutional institutions to Russia. Thackeray presents sufficient evidence to contend that Alexander's understanding of the constitution was limited to its function in regulating the activities of state organs. Alexander failed to understand the supremacy of fundamental law over the will of the ruler and never consistently considered restrictions of autocracy. Ample examples are used to show Alexander's disregard for the constitution of the Congress Kingdom and his apodictic willfulness.

Thackeray is also attracted to a deeper assessment of the work of Alexander's helpers, particularly of Ambassador N. Novosiltsov, and of the discrepancies between Alexander's intentions and those of the thick layer of nationalistic reactionaries at the imperial court who were influenced by the atmosphere of their group to support policies contrary to the liberal dreams of the tsar and the aspirations of the Poles.

The position and personality of Grand Duke Constantine Pavlovich are presented with fresh insight. In the author's opinion, Constantine's marriage to Jolanta Grudzinska was not a major reason for his resignation from the Russian succession. Rather, his general inability to balance the obligations facing the tsar decided his removal from the succession. His increasing authority in the Polish kingdom and the growing extension of his power over the Lithuanian lands are seen as a sort of remuneration for the resignation from the succession in St. Petersburg.

A thorough and sympathetic attention is paid to the Polish parliament and to Adam Czartoryski in his growing disappointments with Alexander's disregard for the constitution and for the justified aspirations of the Polish nation. Within this framework, the submissiveness of the lord lieutenant J. Zajaczek and the feisty parliamentarism of the brothers Niemojowski reflect a permanent tension in the struggle of that nation against the oppressive imperialism of Russia.

The book is based broadly on primary sources, both printed and archival, and on secondary works, and the mastery of this material is visible in the author's analysis of the subject. One can, however, feel some doubt as to whether the exclusion from more detailed consideration of Alexander's grand design for Europe, important in the first years of Alexander's reign and revived at the Congress of Vienna, did not narrow the perspective in the analysis of Alexander's foreign policies and in understanding his concern for the kingdom of Poland.

This is a solid piece of research, thorough, innovative, and well written. Sparse misprints, perhaps possibly avoided by more careful proofreading by the editors, do not bother the reader's perception of the discussed issues.

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R. F. LESLIE, editor. *The History of Poland since 1863*. (Soviet and East European Studies.) New York: Cambridge University Press. 1980. Pp. xii, 494. \$45.00.

This volume edited by R. F. Leslie is a long-expected continuation of the pioneering *Cambridge History of Poland*. The book covers the history of modern Poland from the uprising of January 1863 to most recent times. On the whole, the present reviewer agrees with the treatment of the controversial and complex subject.

Two observations, however, should be noted. The first concerns the style, the second some of the gaps in the content of the book. The treatment of some

of the most dramatic events of modern European history is flat and undramatic. Perhaps it is due to the fact that the authors overlooked some of the most heroic events of the period 1939-45. After all, throughout World War II, while resisting the German as well as the Soviet invaders at home, the Poles continued to fight abroad. For instance, in the Battle of Britain, two Polish fighter squadrons played a crucial role. The small but intrepid Polish Navy took a gallant part in some of the most crucial operations, including the Northern convoys to Mermansk and the sinking of the "Bismarck," and thus earned, per capita, more British and Allied awards than any other navy. Two Polish army corps fought in the Allied invasion of Europe in 1944, one in France and the Low Countries, another in Italy, scoring impressive victories at Falaise and capturing the seemingly impregnable fortress of Monte Casino. Neither of these battles is mentioned in the book.

At the end of the war the Polish government in London had at its command some 200,000 men. If one adds to this the nearly equally numerous Polish forces coming from the USSR and the Polish underground movement of some 300,000 people, the Polish component of the Allied forces ranks in fourth place, behind the USSR, the United States, and Great Britain, but well ahead of far larger and less war-ravaged France.

But by far the most important contributions the Poles made to the Allies' victory over the Axis powers was in the field of military intelligence. After the fall of France at the end of June 1940, the Poles who had refused to surrender but had not been evacuated to Great Britain organized an intelligence net, known as F2. The net rendered invaluable services not only to the French but to the Allied cause as a whole. Here one should mention also the supplying by the Polish underground movement in Poland of the original plans for the German V-rockets, as well as their captured parts.

The most remarkable Polish contribution in the field of intelligence, however, was the reconstruction of the top secret and seemingly impregnable German cyphering called "Enigma." On the eve of the war a team of Polish mathematicians and engineers built 15 copies of "Enigma." By July 1939 the French and the British were presented by Warsaw with one duplicate of this coding device with detailed instructions how to operate it. The breaking of the German code allowed the Allies to read most of Hitler's orders to his field commanders as well as reports of his subordinates to him.

"Enigma" was the machine that gave the Allies what Churchill called "my most secret source of enemy intentions," and its crucial contribution to the victory over the Axis powers is now an axiom. Yet "Enigma" is not mentioned in the book. Let us hope that the next edition of this badly needed



book will correct these omissions and thus make Poland's contemporary history more complete.

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KAI VON JENA. *Polnische Ostpolitik nach dem Ersten Weltkrieg: Das Problem der Beziehungen zu Sowjetrußland nach dem Rigaer Frieden von 1921.* (Schriftenreihe der Vierteljahrshefte für Zeitgeschichte, number 40.) Stuttgart: Deutsche Verlags-Anstalt. 1980. Pp. 243. DM 22.

The current political crisis in Poland has again brought to the fore the precarious nature of Polish-Soviet relations. The problem is an old one, transcending the geopolitical situation in Eastern Europe of the last 37 years. The Bolshevik revolution in Russia and the rebirth of the Polish national state in November 1918 created new dimensions in the relationship between the two neighbors and formed the background for future problems. Both states faced dilemmas whose solutions would have a crucial importance for their future existence.

The Poles had to define the place and role of Poland in the major political conflict of the time: Communist revolution versus capitalism. The relationship with Soviet Russia would not only have an impact on Poland's interactions with the West but most importantly on its territorial integrity and the shape of its borders, in the east and in the west.

Kai von Jena focuses on a crucial period in the development of the new Polish-Soviet relations, the years from 1921 to 1923. In this monograph he utilizes available references and documentary collections with imagination and diligence. Jena first describes the background of the Polish-Soviet Treaty at Riga, giving brief and lucid discussions of Roman Dmowski's and Jozef Piłsudski's political concepts of the eastern border and relations with Russia, Lenin's view of the Polish question, and the Polish-Soviet war. The author then offers a comprehensive discussion of the practical realization of the Little Entente concept, the impact of France on Polish attitudes toward Russia, the Karachan-Dabbski talks, Poland's interactions with the Baltic states, and Polish-Soviet trade relations.

Jena understands the dilemmas of the Polish government. He successfully portrays the one-sided rigidity of Piłsudski's federalist concept coupled with the political limitations of Poland's Baltic counterparts, unrealistic pressures from the French, and Soviet dogmatism and insecurities.

The monograph's narrow focus does some injustice to the discussion of the attitude toward Poland among the Bolshevik leaders. The reader certainly could have benefited more if the author

mentioned divergent views, especially those represented by prominent Poles. This is particularly true of Feliks Dzierzynski, who, contrary to the widely shared view, was not just another figurehead (see, for example, *The Modern Encyclopedia of Russian and Soviet History*, vol. 10 [1978]). This and other small deficiencies do not seriously mar the highly competent, readable, and scholarly narrative, which, because of a comprehensive bibliographical essay, biographical sketches of 27 major political figures discussed in the monograph, and the index, will certainly become a very useful reference tool as well.

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ROBERT K. MASSIE. *Peter the Great: His Life and World.* New York: Alfred A. Knopf. 1980. Pp. xii, 909. \$17.95.

This big book by Robert K. Massie raises questions at several levels of concern to historians, the most basic being: is it history? From the academic (or scientific) point of view, certainly, it is not. Beginning at the back of the book we notice that its comprehensive index does not mention, among many such instances, either Heinrich Fick, who was Peter I's key advisor in devising his famous administrative reforms, or the *Preobrazhenskii Prikaz*, which was the first of the central offices founded by Peter and the first in Russian history to be charged with suppressing political dissent. We notice, moving forward, that scarcely 10 percent of the 855 notes to the text refer to Russian sources, while more than a fourth of the total cite Eugene Schuyler's *Peter the Great, Emperor of Russia: A Study of Historical Biography* (1884). The "Selected Bibliography," coming next, lists indiscriminately other antiquated histories, popular biographies, collections of official documents, contemporary memoirs, textbooks, volumes of unattested anecdotes, and a few scholarly studies, while omitting many more important works in Russian and even in English than it includes. The serious critical and factual deficiencies thus indicated are amply displayed in the text itself, which is divided like a bedtime serial into sixty-four chapters averaging twelve pages each. Nor is the claim to have done extensive research in the Soviet Union or in major libraries in the United States, Britain, and France (p. 861) borne out by a reading of the text.

Massie's book also exudes a remarkable naiveté. He has surrendered to the romance of Russia without apparent resistance, as he has to the legend of Peter the Great, which his book will help to perpetuate. "He was a force of nature," Massie at last concludes, "and perhaps for this reason no final judgment will ever be delivered. How does one judge



the endless roll of the ocean or the mighty power of the whirlwind?" (p. 855). Similarly, in exonerating Peter from some of the most damning charges of inhumanity that can be laid on his tomb, Massie tergiversates, as witness his treatment of the question of torture (pp. 252–53) or of the case of Peter's son and heir, Tsarevich Aleksei (pp. 708–10). Here, the facile relativism as well as the naiveté of the amateur historian is in full bloom.

The specialist cannot in good conscience recommend this book to fellow historians. Yet it would be misleading as well as unfair to leave it at that. For Massie's work strikes this reviewer as, at the same time, an outstanding example of that somewhat ambiguous literary genre known as popular (or commercial) history—as an example, too, of what Norman Mailer must have had in mind when he complained recently that history has become more novelistic than the novel. A colorful, dramatic, at times gripping story is told here in fine detail and in effortless prose. The book is flawlessly printed (in Times Roman) and generously, if not always accurately, illustrated. It far surpasses, in volume rather than in acumen or grace of style, the other popular biographies of the first Russian emperor—by Alex de Jonge and by M. S. Anderson—to have been recently published in English. It will serve to advance the cause of serious history, more likely than not, among the general public; and for that we can only be grateful.

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NINA N. BASHKINA *et al.*, editors. *The United States and Russia: The Beginning of Relations, 1765–1815*. Washington: Government Printing Office. 1980. Pp. lxxxii, 1,184. \$24.00.

N. N. BASHKINA *et al.*, editors. *Rossii i SShA: Stanovlenie otnoshenii, 1765–1815*. Moscow: Nauka. 1980. Pp. 751. 10 r.

This work is remarkable for its size, content, quality, origin, and modest price. There are 560 documents, ably selected, edited, and translated, mostly complete, 60 percent never before published; there are over a hundred portraits and other illustrations, some in color, and several maps. The result is not only a superb collection of source materials on a wide range of subjects but also a fascinating portrayal of forgotten times and personalities. There are abundant notes, a lengthy bibliography and list of archival manuscript sources, and a good index. And, unexpectedly, it is a joint American-Soviet venture, published simultaneously in English in Washington and in Russian in Moscow.

The project originated in an agreement on cultural cooperation signed in 1973, and this particular work was proposed at the Eighth International Congress on Archives held in Washington in October 1976. Representatives of the Main Archival Administration of the USSR Council of Ministers and the National Archives of the United States agreed to prepare a joint documentary publication on the early development of Russian-American relations. A joint editorial board was then formed, each side organized a group of editors, and in less than four years the task was completed. The American editors were John H. Brown and J. Dane Hartgrove of the Diplomatic Branch, National Archives, and Ronald D. Landa and Charles S. Sampson, Office of the Historian, Department of State. The Soviet editors included Nikolai N. Bolkhovitinov, well known for books on early Russian-American relations.

The first documents concern efforts of Ezra Stiles and Benjamin Franklin in 1765 to correspond on scientific matters with their Russian counterparts Lomonosov, Braun, and Aepinus. Contacts thereafter, individually and through learned societies, became increasingly frequent.

Diplomatic and consular materials receive the most attention. Russian diplomats in Paris and London are shown to have sent home accurate and detailed reports about events leading up to the revolution in North America. The measures of Catherine II, although inspired by national interests, were of some assistance to the rebelling colonies. In 1775 she sidestepped George III's request for 20,000 troops for service in America. In 1780, she promulgated her Declaration of Armed Neutrality and in 1781 joined Austria in an attempt to mediate the conflict between Great Britain and its European and American opponents. American envoys assessed these measures realistically and urged that the young republic would have to stand on its own feet.

Considerable attention is given attempts to establish diplomatic representation, starting with Francis Dana's abortive mission to Russia in 1781–83 and the failure of the U.S. in 1795 to get John Miller Russell accredited as its consul in St. Petersburg. In 1803 the first U.S. consul, Levett Harris, acted as go-between in correspondence of Alexander I with Thomas Jefferson; in 1804 he helped secure Russian intervention with the Ottoman Porte on behalf of the crew of the captured U.S. frigate *Philadelphia*. In 1808, Alexander I appointed Andrei I. Dashkov to be consul general at Philadelphia and charge d'affaires in the U.S., and in the following year John Quincy Adams was appointed U.S. minister to Russia and Count Fedor P. Pahlen became the first Russian minister to the United States.

Correspondence of Dashkov, Pahlen, and other Russian diplomatic and consular representatives conveys their impressions of the United States and

its government, and it shows the attempts to solve problems existing between the two countries. Emperor Alexander's attempt to mediate the Anglo-American war of 1812–14 caused Adams to predict that Alexander would become the "umpire" of Europe and would exert influence beneficial to the United States.

Documents on Russian activities on the Pacific coast of North America, including some not previously published, start with G. I. Shelikhov, a Siberian merchant who founded the first permanent Russian settlement in North America and whose ventures led to establishment of the Russian-American Company under a monopoly in 1799. Dealings between Aleksandr A. Baranov, long-time manager of the Russian settlements, with the "Bostonians" (maritime traders from New England and New York) eased persistent supply problems; but the Americans also undercut Russian trade and sold firearms and liquor to the natives, endangering Russian settlers. Diplomatic negotiations failed to bring a solution. A short-lived accommodation worked out by the Russian company with John Jacob Astor's American Fur Company was soon nullified by the American war with Great Britain.

Many explorers, travelers, and soldiers of fortune appear in the pages of this collection. John Ledyard, who sailed with Cook in 1778, described the first meeting of an American with Russians on the Northwest coast, and in letters of 1787–88 he recounted a journey through Siberia, from which he vainly hoped to reach and cross North America. The first Russian travelers on the east coast were naval officers training in the British navy. A long extract from Lt. Iurii F. Lisianskii's unpublished journal deals with his travels through the northeast and Middle Atlantic states in 1794–95, during which he met George Washington. Narratives by Americans in Russia include an extract from the unpublished journal of Peter Allaire, who was in St. Petersburg in 1774; a long letter from sixteen-year-old John Q. Adams to his mother in 1783; and letters from Joseph Allen Smith to Rufus King (1802–05), Joel R. Poinsett (1806–08), Alexander Hill Everett (who accompanied John Q. Adams to St. Petersburg in 1809), and Peter Dobell, an Irish-born American who tried to develop trade in Kamchatka with America and the Orient.

Materials on military service include documents dealing with the Revolutionary War service of Baron Gustavus Heinrich Wetter von Rosenthal of Estonia and the service of John Paul Jones in the Russian navy during the Ochakov campaign of 1788. Commerce is dealt with in documents and notes on the first American ships that ventured to St. Petersburg in the 1760s; other materials depict subsequent economic relations. Pavel P. Svin'in, secretary with the Russian legation at Philadelphia

in 1809–13, wrote lengthy descriptions of American trade. (Several of Svin'in's paintings of American scenes are reproduced in color.)

The Russian volume corresponds closely to its American counterpart but is thinner because of a shorter introduction, narrower margins, and smaller print. It will be of use to researchers desiring the original Russian texts.

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MURIEL ATKIN. *Russia and Iran, 1780–1828*. Minneapolis: University of Minnesota Press. 1980. Pp. xii, 216. \$20.00.

Russia's annexation of eastern Georgia in 1801 inaugurated a decade of rapid territorial expansion in Transcaucasia that involved Russia in war with Iran. A second phase of empire building in the region in 1826–29 was accompanied by wars against both Iran and Turkey. Muriel Atkin's study deals primarily with the origins and course of the Russo-Iranian war of 1804–13, with only a brief epilogue on the subsequent fifteen years. Based on archival research in France and Great Britain and on extensive reading of published materials in Russian, Iranian, English, French, and German, the book contains much useful information on Russia's conquests, on the resultant diplomatic and military clashes between Russia and Iran, on the internal politics of early nineteenth-century Iran, and on Anglo-Iranian and Franco-Iranian relations. Unfortunately, there is no clear focus: each of the above themes at times overshadows the others, and none of them is clearly dominant. The study does not deliver what its title promises, touching only superficially, for example, upon Russo-Iranian trade and paying no attention to the origin and nature of the extraterritorial privileges gained by Russia in Iran in 1828. On the problem that is central to her study—the war of 1804–13—Atkin's conclusions about Iran's motives for going to war are puzzling. She argues that the "real causes" of the conflict are to be sought in the newly enthroned Qajar dynasty's need "to enhance its legitimacy" by reasserting Iranian hegemony over the eastern Caucasus (p. 92), but she then contends that the "specific cause of the war" was the shah's fear that Russia meant to seize parts of Iran proper and reduce his country to political vassalage (p. 95).

Atkin's anti-Russian bias leads her into some interesting but untenable positions. Insisting that Russia's expansion into Transcaucasia was the product of a "colonialist outlook that was consciously imitative of Western overseas expansion" in its belief that the possession of colonies would en-

rich the metropolis and "would prove that Russia, too, was as great and civilized an empire as those of western Europe" (p. 163), Atkin ignores the long history of Russian expansion into areas adjacent to its insecure frontiers whenever the relative strength of Russia and the neighbor in question permitted. Empire building was one area in which Russia had little to learn from the West. Atkin is particularly harsh on Russian officials for their "characteristic inability" to understand the point of view of either Iran or the east Caucasian khanates, "or even that there could be a rationally considered Iranian point of view that differed from Russia's" (pp. 91, 140). She overlooks the likelihood that the Russians, like other imperialists, simply did not think the point of view of the natives worthy of serious consideration. Russians were arrogant but not necessarily obtuse. And Atkin is so anxious to point up every failure of the Russian military, especially those of Prince P. D. Tsitsianov, that the uninformed reader would never recognize in him the conquistador who in the short space of three years subdued most of present-day Soviet Azerbaijan and western Georgia, expanding Russia's original landlocked and tenuously held east Georgian colony into an imperial domain stretching from the Black Sea to the Caspian.

Poor organization makes Atkin's story difficult to follow. Four separate thematic chapters on the period 1804-13 view the same events from different angles at the price of considerable repetition. In the absence of even a brief chronological account of these years as a whole, the result is a tangled web of facts and observations rather than a coherent picture. Almost every chapter in the book begins with a summary of the material to come, replete with references to events not explained until later and with conclusions for which the evidence is yet to be presented. Finally, the study is marred by numerous typographical errors and the following howler: "icebergs made sailing the Caspian perilous during the winter" (p. 103). Atkin's research has yielded much useful information, but her book is far from the definitive treatment of her subject.

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V. A. D'IAKOV. *Osvoboditel'noe dvizhenie v Rossii, 1825-1861 gg.* [The Liberation Movement in Russia, 1825-61]. Moscow: Mysl'. 1979. Pp. 286. 1 r. 10 k.

During the past century, a great deal has been written about the history and intellectual content of the radical movement in Russia between 1820 and 1861. Of course, that does not mean that there is no room for further inquiry or that all the important and interesting questions have been answered or, for that matter, even asked. It ought to mean, how-

ever, that certain basic facts and interpretations are well enough known so that they need not be gone over again in a scholarly monograph. Regrettably, it is the well-trod ground and the already known facts that V. A. D'iaikov covers in this volume.

D'iaikov sets out to survey "the entire aristocratic stage of the liberation movement in Russia, beginning at the point where the ideas of Decembrism matured and ending on the eve of the first revolutionary situation" and to relate the men and ideas of the radical movement in Great Russia to their counterparts among the empire's nationality groups, especially the Poles (pp. 9-10). In a relatively superficial manner, he accomplishes these aims, although he claims far more for the result than the book's content can justify. Certainly, after D'iaikov has completed what he calls "a revelation of the basic typological characteristics of the liberation movement in Russia during the aristocratic stage of its development" (p. 246), we are not left with much other than a reminder of the very obvious point that what began as an aristocratic movement evolved into one in which the *raznochintsy* had begun to assume a major role by the 1860s. It would have been far more interesting and useful if we had been given some better explanations about how and why that well-known evolution occurred.

D'iaikov attempts to apply prosopographical data to his study, a relatively innovative methodological effort within the context of Soviet historical writing. Yet he assembles little data and asks very few questions of the materials he does assemble. It is of very little consequence, for example, to be told that between the 1820s and the 1880s the portion of nobles in the liberation movement diminished "by an average of 7-8% in each decade" (p. 61), when the author does not provide much by way of explanation and we have long known the end result in any case. At the same time, much more could (and ought to) be said about the very important prosopographical differences between the older and younger groups of Decembrists, a critical point that D'iaikov overlooks because he lumps all Decembrists into one large, quite amorphous group. Clearly, he does not bring to his task the methodological sophistication of such Soviet colleagues as P. A. Zaionchkovskii or, especially, the late S. M. Troitskii, and his use of prosopographical data highlights the method's shortcomings, not its virtues. Nonetheless, D'iaikov's volume does have some value for Western readers in that it provides a summary of more recent Soviet studies about Russia's radical movement between 1820 and 1861. As such, it can serve as a useful survey of the "state of the art" in the Soviet historical community about a major problem in imperial Russia's history.

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BARBARA A. ANDERSON. *Internal Migration During Modernization in Late Nineteenth-Century Russia*. Princeton: Princeton University Press. 1980. Pp. xxv, 222. \$18.00.

The volume under review is the latest addition to the now sizable roster of studies devoted in whole or in part to the statistical manipulation of aggregate data contained in the first, and only, national census of the Russian empire, which was taken in 1897. The underlying premises of Barbara A. Anderson's research are that a person's attitudes help to determine propensity to migrate, that they help to influence the choice of destination, and that the physical and socioeconomic milieu of the place of origin play an important role in shaping such attitudes. Although the migration process is broadly differentiated according to whether the destination is urban-industrial, an agricultural frontier, or an intermediate kind, the principal objective of Anderson's research is to establish whether or not there are significant differences in the characteristics of the origins of migrants to each destination type. To this end the *gubernii* (provinces) of Russia are grouped according to broad similarities in level of literacy, industrial development, population density and demographics, and importance of traditional agriculture. The movement of migrants between regions with common ecological characteristics and particular destination types is subjected to correlation and multiple regression analyses—analyses that also include simple distance as an explanatory variable and that differentiate the migrant population according to sex and socioeconomic status. Anderson, a sociologist, acknowledges that this revised version of her doctoral dissertation "is not primarily a work about Russian history" (p. xxiii). As an exercise in testing a particular model of internal migration during modernization, the study serves its purpose reasonably well; there is perhaps reason not to be as sanguine about the utility of the findings for historians of Russia.

After outlining the research problem, definitions, and hypotheses in chapter 1, Anderson describes the destination types and ecological characteristics of origin regions. Chapter 3 analyzes the pattern of out-migration in terms of literacy, population pressure, soil fertility, and industrial development of the *gubernii* of origin. Chapter 4 examines the pattern of migration to Moscow and St. Petersburg, "the most culturally and industrially modern locales in European Russia in 1897" (p. 90). In this analysis, migration data from the one-day city censuses are used to complement the 1897 data base. Migration to the agricultural frontier of Siberia and Central Asia is analyzed in chapter 5, while what are referred to as destinations of intermediate modernity and migration in the Pale comprise the subjects of the sixth

and seventh chapters. The principal findings are summarized in chapter 8.

On the whole, the results of the study conform reasonably well to initial hypotheses. It was only in the case of the relationship between migration and industrial development of the region that divergence occurred. Migration rates from industrially modern origins to industrially modern destinations were less than expected, while migration rates to agricultural frontiers were greater than expected. Thus, ecological characteristics of regions of origins are assumed to influence the decision to migrate and the destination selected.

Although Anderson invokes appropriate caveats to the interpretation of the results and occasionally offers some speculation about the process, some aspects of the study still raise doubts about the utility of the findings. For example, the contention that Moscow was more an industrial center than St. Petersburg in 1897 does not square with the facts. Industrial employment in St. Petersburg was larger and the industrial structure more modern than Moscow's at this time, yet the interpretation of the results of the analysis of migration patterns in this chapter hinges on the classification of Moscow as an industrial center and St. Petersburg as a cultural one. The list of examples could be expanded, but there are sufficient queries about the accuracy of the historical milieu portrayed to put some of the results of the analysis in question. Certainly, as Anderson notes, there is scope for further work still.

The book merits a quick read but more for the questions raised than for the answers provided. The presentation is readable though scarcely lucid. The bibliography suffers from occasional inaccuracy and mistransliteration of Russian sources.

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S. M. SIDEL'NIKOV. *Agrarnaia politika samoderzhavii v period imperializma* [The Agrarian Policy of the Autocracy in the Period of Imperialism]. Moscow: Moscow University Press. 1980. Pp. 286. 3 r. 30 k.

This is the first book to deal comprehensively with the Stolypin land reform since S. M. Dubrovskii's massive study came out in 1963. S. M. Sidel'nikov uses much less prose than Dubrovskii did, but he manages to cover approximately the same ground. His description of the central government's policies throughout the period 1902–14 is considerably clearer and more complete than Dubrovskii's. In particular, the chapters on peasant migration to Siberia add much material to existing accounts of the reform, although most of it comes from secondary sources. As to the reform in European Russia, the author should be praised for recognizing that the



conversion of peasant land from communal to personal ownership was not the chief aim of the reform. Perhaps now the conventional view will at last begin to disappear from the textbooks. Finally, Sidel'nikov offers a relatively coherent picture of the work done on reform legislation in the Duma and State Council, although here too he does not get beyond printed sources.

Despite these achievements, the book is disappointing. Sidel'nikov edited a collection of source readings on the Stolypin reform in 1973, and his editorial annotations reflected a much deeper knowledge than his new book embodies. In 1977, I spent six months looking through the State Historical Archive in Leningrad, and it was a rare day when I came across a *delo* he had not seen. Sidel'nikov does know the material. Yet his descriptions of legislation—which take up most of the book—are little more than interminable lists of provisions, important ones heaped together indiscriminately with the trivial. These dull heaps of raw information, often presented without any reference to a source, are unrelieved by any attempt to explain or interpret (except, of course, for an occasional reminder that the government was hostile to the people). One recognizes the need for Soviet historians to kiss Lenin from time to time, but Sidel'nikov does very little else by way of interpretation. For example, he fails to point out that the Ministry of Agriculture took over the reform from the Ministry of Internal Affairs during the course of implementation, although his paragraphs contain the requisite information. Then, too, there are some outright omissions. Sidel'nikov never once mentions the growth of rural cooperatives during the Stolypin years. Worse, he leaves out all reference to group land settlement (measures to reduce interstripping on cultivated land that fell short of full consolidation into integral farms). Reading Sidel'nikov, one would not guess that credit cooperatives and group land settlement even existed, so of course Sidel'nikov cannot tell us that they were becoming the two key developments in the Stolypin reform by 1914. I suspect he knows it. Something or someone seems to have hampered his writing.

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U. A. SHUSTER. *Peterburgskie rabochie v 1905–1907 gg.* [St. Petersburg Workers in 1905–07]. Leningrad: Nauka. 1976. Pp. 281. 1 r. 46 k.

U. A. Shuster's five-year-old monograph delineates the role that the working class of St. Petersburg played in Russia's first great revolutionary upheaval. Shuster describes the condition of the capi-

tal's workers at the beginning of this century, their involvement in Father Georgii Gapon's Assembly of St. Petersburg Factory Workers, their response to Bloody Sunday, the steady development of the workers' movement in the city until the October general strike, the triumphs and failures of November and December, and the gradual decline of the movement in 1906 and 1907.

Shuster's analysis of several topics makes this book valuable to students of both labor history and the 1905 revolution. He shows how the arbitrary definitions employed by tsarist bureaucrats prevent an accurate count of both workers and factories by present-day scholars. In his discussion of working-class society, he points out that the city's workers, although nominally peasants, had severed most economic ties to the village but had not yet produced a "second generation" of hereditary laborers. Shuster's evenhanded treatment of Father Gapon and his union also deserves praise. Gapon emerges as an ambitious man, whose flaw made him subject to manipulation by the "inner circle" of the assembly's working-class leaders rather than by the Okhrana. Finally, the author emphasizes the spontaneous birth of the St. Petersburg Soviet and the natural expansion of its authority as workers, peasants, and even other soviets turned to it for leadership.

These strong points, however, have corresponding flaws. Shuster indicates that the vast majority of St. Petersburg workers left wives and children back in the village, yet he de-emphasizes this fact in his eagerness to present them as a true proletariat; for a more enlightening treatment of this subject, one must turn to Robert E. Johnson's *Peasant and Proletarian: The Working Class of Moscow in the Late Nineteenth Century* (1979). Shuster completely ignores the existence of that famous nonperson Leon Trotsky, probably the single most important figure in the St. Petersburg Soviet. As a result, the political stance of the soviet is all but incomprehensible. In general, whenever the author drags the Social Democrats and their policies into the text, the discussion becomes cliché ridden and ponderous. The reader can easily skip over these parts without impeding his understanding of the book's argument or interrupting the narrative flow.

Because these weaknesses are predictable, they do not completely undermine Shuster's analysis. Anyone interested in the revolution of 1905 should look at this able monograph.

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A. S. MOSKOVSKII. *Rost kul'turno-tehnicheskogo urovnia rabochikh Sibiri, 1920–1937 gg.* [Growth of the Cultural-Technical Level of the Workers of Siberia, 1920–37]. Novosibirsk: Nauka. 1979. Pp. 347. 2 r. 20 k.

When communist rule began in Russia in 1917, most of the population was illiterate and few industrial workers were skilled. The story of how the Soviet regime struggled to change this situation in the shortest possible time is well worth studying. A. S. Moskovskii, who has written several other studies of Siberian workers, has provided in the present work a valuable history of these efforts in Siberia from 1920 to 1937.

Developing countries of the third world may find in this account genuinely useful examples of how to overcome obstacles to their own progress. The author also devotes much of the book to accounts of campaigns to destroy religious sentiments and instill "correct" Marxist-Leninist views, which will doubtless prove less useful. Moskovskii's study of the campaign against illiteracy is particularly interesting, as is his account of how the school system was used to educate adults as well as the young. He provides much useful statistical information. In Siberia as in other parts of the USSR the proficiency of industrial workers was increased by formal training, supervision by senior workers of their juniors, on-the-job training, "socialist competition" among groups of workers, and encouragement of worker inventiveness and suggestions.

The author devotes much of the introduction to a review of existing scholarly secondary literature relating to his subject. Curiously, in this bibliographic essay Moskovskii ignores his own 1968 monograph on the formation and early development of the Siberian working class, although he does refer to it later. The present work is in fact an amplification of chapter 3 of that book.

Moskovskii has made good use of a wide variety of published and archival source material. As an example of current Soviet historiography this book is both encouraging and disappointing. It is encouraging to find an insistence on critical use of the sources, but his lengthy discussion of ideological campaigns treats Communist party statements as actual fact, although reports of workers *voluntarily* donating their wages to the party seem questionable. It is also discouraging that, in a book devoted to Soviet history from 1920 to 1937, Stalin's name appears only twice—each time in a quotation! There is no mention at all of the secret police or of the corrective labor camp system. The full story remains to be told.

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ROY A. MEDVEDEV. *Nikolai Bukharin: The Last Years.* Translated by A. D. P. BRIGGS. New York: W. W. Norton. 1980. Pp. 176. \$10.95.

Roy A. Medvedev deals with the last ten years of Bukharin's life, when his influence on Soviet policy gradually waned and the "party's favorite son" became a conspirator, turncoat, enemy of the people, and eventually an "unperson." Simultaneously, his Leninist (NEP) goal of a peaceful evolution of Soviet society toward socialism was denounced as a right-wing deviation. Medvedev uses his inside knowledge to bring freshness and warmth to his narrative. In the background looms the sinister figure of Stalin, whom Bukharin regarded (Medvedev insists) as his friend almost to the bitter end—his vigilance blunted by the occasional token of benevolence and his own continued enjoyment of party office and privileges. He had to abandon his plans for the peasantry, was removed from the editorship of *Pravda*, and was repeatedly attacked in the press until he recanted at the Seventeenth Party Congress in 1934 and was appointed editor of *Izvestiia*.

Early in 1936, with Stalin's valediction, Bukharin was sent abroad to contact foreign politicians about the safekeeping of the Marx-Engels archive. He met émigré Mensheviks such as Boris Nicolaevsky and Fedor and Lidia Dan, whose written recollections of their conversations with him are vital to our understanding of his tragedy. Medvedev dismisses these accounts as "conjecture," although he concedes in the next breath that, after reading Nicolaevsky's book, he regards him as largely reliable. Non-Soviet researchers who have consulted Lidia Dan's original notes will continue to regard her as no less trustworthy, even when she reports Bukharin's calling Stalin a malicious devil, forecasting his own probable doom, and deciding nevertheless to return home (perhaps hopeful even at that stage of influencing the course of events there).

On an expedition to Central Asia soon after returning, Bukharin learned that Kamenev and Zinoviev had denounced him at their trial as a crypto-oppositionist and that his close associate Tomskii had committed suicide. Medvedev records the last few months preceding his arrest with compassion. He believes Bukharin never abandoned his Marxist-communist faith, conjuring his wife Larina to rear their son as a true Bolshevik.

Turning to the trial, Medvedev starts by launching an unwarranted attack on Aleksandr Solzhenitsyn's character, taxing him with excessive harshness in condemning Bukharin's undignified posture as a defendant. But when Solzhenitsyn wrote of the trial, he was familiar only with Soviet reports of the proceedings, which admittedly depict his behavior as quite abject. Closer study of the stenographic record of his deposition and final plea reveals that he used Aesopian language; while admitting general guilt, he refuted every single concrete charge made

against him. In the light of this realization, his conduct appears entirely different.

Medvedev himself points to a double meaning in Bukharin's last public utterances but does not venture to decipher the riddle. In his afterword he urges us to continue our analysis of these events. He might have told us more about the last letter Bukharin wrote before his execution—he mentions the drawer in which Stalin kept it, quotes the opening sentence, but leaves it tantalizingly at that. Such reticence should warn us to view his interesting but puzzling book with a detached and critical eye.

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V. IA. SIPOLS. *Diplomaticheskaiia bor'ba nakanune vtoroi mirovoi voiny* [The Diplomatic Struggle on the Eve of the Second World War]. Moscow: Mezhdunarodnye Otnosheniia. 1979. Pp. 319. 1 r. 80 k.

This work was published to mark the fortieth anniversary of the beginning of World War II. It is a reaffirmation of the traditional Soviet interpretation of the diplomacy of the 1930s. V. Ia. Sipols, a Latvian historian and long-time student of Soviet foreign policy, never deviates from the Communist party line on the origins of the war. The Soviet Union, he tells us, having identified the great danger inherent in fascist imperialism, was a constant advocate of collective security. The Western powers, motivated by inextinguishable class hatred of the revolution in Russia, rebuffed every effort made by Moscow to promote the common defense and sought to turn Nazi aggression eastward against the Soviet Union. Only on the eve of war, with the Western powers still resolutely refusing military co-operation, did Moscow, as a last resort to ensure Soviet security, accept the German offer of a non-aggression pact.

In this tale there is no room for chance. Fascist imperialism and the Western conspiracy against the Soviet Union unfold relentlessly and methodically. Nor, apart from class hatred, is there any explanation of why the Western powers found the prospect of cooperation with the Soviet Union so repulsive. Nothing is said of the great purges, their impact on the Soviet armed forces, or the terror inspired by Joseph Stalin. The general secretary, in fact, rarely appears in this narrative, and a novice to the field, simply reading this book, could be excused for not grasping the importance of Stalin to the origins of the war. Sipols paints a faceless and impersonal picture of Soviet foreign policy, portraying it as the work of the "Central Committee of the Communist Party of the Soviet Union." There is

nothing faceless about Western policy; the gaunt features of Neville Chamberlain stare from every page with all his warts exposed to public view. Sipols, who had access to the British cabinet papers, has no difficulty in cataloging the sins of appeasement. Ironically, he does not appear to have had equal access to the secret papers of the Soviet government and is reduced to citing secondary sources as evidence for many of the most important decisions made in Moscow just prior to the outbreak of the war. It is clear, therefore, that the Nazi-Soviet pact of August 1939 remains as great an embarrassment to Moscow as ever and cannot yet be dealt with openly. Needless to say, there is no mention here of the secret protocol to that pact partitioning Poland and the remainder of Eastern Europe.

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## NEAR EAST

ROY P. MOTTAAHEDEH. *Loyalty and Leadership in an Early Islamic Society*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1980. Pp. x, 209. \$16.50.

The Muslim world of the tenth and eleventh centuries was undergoing a politico-military convulsion in which the 'Abbāsid caliphate lost most of its power, remaining a symbolic institution that was tolerated by ambitious leaders who carved territories for themselves and had the caliph grant them legitimacy. Petty dynasties and Shī'ism had achieved political dominance in Egypt, Syria-Palestine, the Arabian peninsula, and elsewhere. In 945, the caliph al-Mustakfi (944-46) received Aḥmad Ibn Būyid and made him his *amīr al-umara'* (commander of commanders) with the honorific surname of Mu'izz al-Dawlah (the one who gives might to the dynasty), that is, to the 'Abbāsid dynasty. That was a fiction over which the caliph had no choice, and the Shī'ite Aḥmad ruled Western Iran and Southern Iraq paying lip service to the caliph whom he blinded one year later.

The work under review is concerned with the Būyid dynasty (945-1055) and aims at singling out and classifying the various social groups with reference to vested interest, internal ties, and the mechanisms binding them to the established authorities. In a lengthy introduction, Roy P. Mottahedeh reviews "the safeguards" of the interest of the Islamic community as implied in the religious law and with reference to some theoretical and practical aspects of succession. The second chapter discusses "acquired loyalties" describing the importance of political favors or benefit (*ni 'mah*), oaths, compacts, and

patronage by which loyalties and obligations are achieved. The third chapter deals with "loyalties of categories" pointing to the different social groups: the nobility and the common people, soldiers, merchants, professionals, and other marginal factions. The fourth chapter describes "kingship and society."

Although the author is inclined to think that the granting of benefit, oaths, compacts, and other mechanisms was conducive to a stable social order, it appears clear from the study that the social structure under the Būyids lacked cohesiveness or a common denominator that would bind the different groups and that the relationship between the people and the government was tenuous at best, allowing for the fragmentation of the central authority as embodied in the caliphate. This deterioration was a common feature under the 'Abbāsids, and the Būyid period may serve as a case study. In this respect, the author fails to relate it to previous or subsequent developments, leaving the reader with the impression that the social structure was a peculiarity of the Būyids only. For the rest, the work constitutes a useful study and gives an insight into an important period of Islamic history. It is well researched and based for the most part on Arabic and Persian sources that are used prudently and includes interesting and illuminating anecdotes. It makes interesting reading and avoids the jargon used in the social sciences. Nonetheless, it is marred by some typographical errors in the transliteration of Arabic terms and lacks a general and comprehensive index.

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JOHN L. ESPOSITO, editor. *Islam and Development: Religion and Sociopolitical Change*. (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1980. Pp. xix, 268. Cloth \$22.00, paper \$9.95.

Comprehension of recent events in Iran and other Islamic countries is greatly facilitated by the excellent scholarship of this collection of essays. Michael C. Hudson presents a general examination of Islam and political development. John Thomas Cummings, Hossein Askari, and Ahmad Mustafa assess the adaptability of the Islamic tradition to economic development. The other essays deal with individual countries. Daniel Crecelius focuses on secularization in Egypt from Muhammad Ali to Sadat. John Alden Williams investigates veiling in Egypt. Mangol Bayat treats Islam in Pahlavi and post-Pahlavi Iran. Yvonne Haddad explores the mutual influences of the Arab-Israeli wars, Nasserism, and Arab and Islamic politics in general. James P. Piscatori analyzes the uses of Islam made

by the Saudi monarchy and reflects on the possible future impact of Islam on the monarchy. John L. Esposito deals with the limited results of the attempt to articulate the Islamic identity of a country that was created as a Muslim homeland, Pakistan. Fred R. von der Mehden writes on Malaysia, Akbar Muhammad on Nigeria, and Lucy E. Creevey on Senegal, three countries where Muslims constitute the major component of the population but where Islam is less prevalent and ethnic and cultural diversity is more marked than in the other countries.

The common point of departure, explicitly or implicitly, is the view, which the authors believe is dominant in Western thought, that secularization is causally related to modernization. All the contributors agree that secularization has not advanced as far as has commonly been believed. They appear to this reviewer to have somewhat differing views concerning the questions of whether secularization and modernization are inseparable and whether Islam can secularize. The contributors employ varying approaches and concentrate on the special features of their individual subjects, but there are a number of common points. Islam, most of the essays make clear, is not monolithic. Instead, many competitive actors and institutions identify themselves as Islamic, and Islam, in varying formulations, nourishes ideology for all. A central issue everywhere is the relation between the state and the religious institution. In most cases, the state has established its supremacy over orthodox religious organizations, but the latter, as shown recently in Iran, have potential of their own, and everywhere various elements outside both state and religious institution mobilize and organize in the name of Islam. Space does not permit calling attention to the special features of individual essays. It seems worthwhile, however, to mention Williams's conclusion that the recent popularity of Islamic dress in Egypt represents the efforts of individuals to meet the intense personal stresses resulting from contemporary social problems.

All the essays impress this reviewer, whose competence is limited to the Near East, as thoughtful studies based on sound scholarship. The essays on the Near East contain much original material and are of value to both the specialist and the non-specialist. As diverse strong partisan attachments are reflected in much of the recent scholarship on Iran, Bayat's analysis should be read by all those interested in this subject.

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WILLIAM OCHSENWALD. *The Hijaz Railroad*. Charlottesville: University Press of Virginia. 1980. Pp. xvi, 169. \$14.95.



The eighteen-year history of the Hijaz railroad is undoubtedly the most remarkable, the most romantic, and yet the most elusive of any railroad in the modern world. Built as a major military, religious, and prestige project by Sultan Abdel-Hamid II, it enjoyed only five years of peacetime operations (1909–14) under the Young Turks and another three and a half years of wartime traffic before the last through train made its slow way from Damascus to Medina in April 1918. Its main records appear to have been destroyed a few months later, along with many of its bridges, most of its rolling stock, and, of course, the Ottoman empire itself.

How then is its success or, just as important, its place in Middle Eastern history to be judged? William Ochsenwald has worked diligently through both the Ottoman and the European sources to find what little hard information there is about the construction, financing, and operation of the railroad without ever really coming to grips with its larger significance. The easiest part to examine is the financial aspect. Unlike any other major project undertaken during the last years of the empire, the records show that it was paid for almost entirely out of local resources, about a third of which were donations and the rest taxes. An assessment of the efficiency with which the railroad was constructed and then managed is much more difficult. For one thing, the sources are too fragmentary and the period under discussion too short to form any balanced view. There is also the point that the track was a narrow gauge, something that makes comparison with most other Ottoman railroads unsatisfactory, and that, in effect, it consisted of two quite different enterprises: the much-used branch line from Deraa to Haifa and the longer stretch south to Medina that was employed almost exclusively for the transport of soldiers and pilgrims. In Ochsenwald's opinion, the cost of construction was reasonably low and the management of the railroad reasonably efficient. But, sadly, the evidence for these assertions is very thin.

Some of the same problems affect the author's efforts to examine the impact of foreign assistance. It was certainly the sultan's original intention that it should be constructed and operated by Ottoman officers and government officials and that the bulk of the rails and rolling stock should be manufactured in Ottoman arsenals. But within a few months of starting in 1900 it was soon realized that European expertise would be required if construction was to proceed at more than a snail's pace. Once again the records are too skimpy to permit Ochsenwald to draw up a satisfactory balance sheet between Ottoman and foreign inputs or to show how successful the Turks were in reducing their dependence on Europe over time.

As for the larger question of its place in Middle

Eastern history, Ochsenwald seems unsure about what frame of reference to apply. Is it best seen as a symbol of Ottoman self-assertion, a test of local expertise, an important strategic and commercial artery, or perhaps—a theme that he studiously ignores—simply a plaything of such larger-than-life historical actors as the sultan himself, T. E. Lawrence, and the Emir Abdullah? What this useful work seems to lack is a point of view.

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BARRY RUBIN. *The Great Powers in the Middle East, 1941–1947: The Road to the Cold War*. Totowa, N.J.: Frank Cass. 1980. Pp. xiv, 254. \$26.00.

This short book includes all-too-brief overviews of the modes of American policy, oil rivalry, and Anglo-American relations, case studies of Anglo-American conflict over Saudi Arabia and great power conflict over Iran and Turkey, a brief chapter on U.S. perceptions of Soviet Middle East policy, and an examination of postwar crises in Iran and Turkey that led to the Truman Doctrine.

Barry Rubin's summary of various U.S. policies toward the Middle East makes effective use of OSS records, and his analysis of Anglo-American relations in Saudi Arabia provides insight into Saudi Arabia's attempts to manipulate Great Britain and the United States. As an overview, however, the book suffers from two deficiencies: insufficient scrutiny of the public record and a failure to reflect the findings of many excellent monographs on the region that have appeared in recent years. As a consequence, it sometimes reveals a superficial understanding of crucial events (for example, the two Iranian crises of 1946) and developments (for example, the relationship between various events, bureaucratic perceptions, and the formulation of policies), analysis of which is often overshadowed by 1,300 lines of quotations not always judiciously selected from cables, internal memos, and briefing books.

Although a few factual errors creep into the text (for example, Eisenhower, whom Rubin portrays questioning Truman during an important meeting on Turkey in 1946, in fact never attended the meeting), they are inconsequential relative to the book's major flaw: its conceptualization. If its focus is on developments that contributed to the Cold War, why devote time to extraneous issues such as Anglo-American relations in Egypt and exclude Anglo-American relations in Greece? Greece, after all, came under the jurisdiction of NEA, and even if the State Department now considers it part of Europe, its role in the Near East was central to British and

American strategic thinking in the period 1944-47. This oversight leads Rubin to underestimate British concern about Russia during the war and err in his judgment of when the strategy of moving the U.S. closer to Britain in order to counter Soviet ambitions in the Middle East was first voiced. In short, the book is held together less by a carefully developed thesis than by a geographical expression—whose components provide a variety of different contexts for great power rivalry but whose function as an organizing frame of reference is inadequate for a problem that transcends its boundaries.

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BARRY RUBIN. *Paved with Good Intentions: The American Experience and Iran*. New York: Oxford University Press. 1980. Pp. xii, 426. \$17.50.

Barry Rubin's *Paved With Good Intentions: The American Experience in Iran* excellently chronicles America's diplomacy from the beginning of the constitutional movement in that country (1905-06) to the present time. It is an insightful and objective look at a relationship that has been complicated, controversial (both from the Iranian as well as American points of view), and often based on mutual misunderstandings and distrusts. The thrust of this study is an explanation of the revolution in Iran, the role of the United States in, and response to, that revolution, and the disastrous effect that it has had on this country's status in the Persian Gulf region.

According to the author, the political events of the early 1950s are no more than ancient history to most Americans; to most Iranians, however, the period was the essential backdrop to their 1978-79 revolution. President Eisenhower and John Foster Dulles tended to see reform movements as disruptive and as likely to be captured by communists. Thus in 1953 they undermined the government of Muhammad Mosaddegh and facilitated the shah's return to power from his short exile. And, although this turnaround was not due solely to decisions taken by the United States (the split with Mosaddegh and his National Front was caused largely by the failures of the nationalists themselves), America's image as the foe of reform was fixed permanently in the minds of most Iranian reformers.

The shah's assumption of power did not resolve the underlying problems of political legitimacy and cyclical economic crisis that had plagued Iran for many decades. Painfully aware of this, American policy makers from 1954 to 1968 sought continually to minimize their commitments to Iran. Not until the late 1960s would Iran's growing oil wealth, its changing strategic situation, and the seemingly successful "White Revolution" of the shah and his

demonstrated ability to maintain order convince American officials that they could do more for the American-Iranian relationship.

By the early 1970s Nixon and Kissinger had inaugurated a turning point in United States policy toward Iran. From the traditional peripheral position Iran emerged as the key pillar of support for American interests in the Persian Gulf region. Disillusioned by the problems of direct military involvement in Vietnam, the "Nixon Doctrine" shifted to an indirect projection of U.S. power through specified Third World allies. Thus with the blessing of the State Department, Iran became the largest buyer of military equipment in the developing world during the 1970s.

The Carter presidential triumph of 1976 changed the nature of the American-Iranian relationship. With Carter's emphasis on "human rights," the total support of the shah was not forthcoming at a time when he, in fact, needed that support more than ever. The overconcentration of wealth and power, wild inflation, extravagant consumption, the rising food import bill, the military's priority over civilian needs, the lack of trained technicians, debilitating corruption, political repression, and the general wishful thinking that substituted for truthful evaluation among those who ran the country had made the shah's position extremely precarious. During this most crucial time, what emerges clearly from the pages of *Paved With Good Intentions* is the picture of an administration totally without a consistent policy.

When late in 1978 millions of demonstrators in the streets of Iranian cities were challenging the regime, the White House, the State Department, the CIA, and the Defense Department each had varied reactions and recommendations. Each step was dictated partly by chance, partly by the relative strength of various personalities in the administration, and very much by whatever turn events might be taking at the moment in Iran. The key decision was finally made by Vance and Carter at Camp David during the Christmas-New Year vacation period (1978-79), and General Robert Huyser was sent to Iran to ask the shah to leave the country. The initiative, however, had passed from the hands of the United States. The action was taking place in Iran; all that America did was to react.

The author points out that the takeover of the American embassy in November 1979 and the taking of the hostages marked the end to one revolution in Iran and the opening shots of a second. For Iran, like Russia in 1917, was to undergo both a February and a November revolution. The first was a struggle to topple the shah's regime, the second, a social and economic and cultural revolution to build a new Islamic society. In this second revolution, the fundamentalist mullahs and their Islamic

Republican party sought to achieve what the Bolsheviks did in 1917—monopolize power. And like Lenin, Khomeini would in time turn against moderates and purge them from positions of authority.

*Paved With Good Intentions* is by far the most accurate and comprehensive account of United States–Iranian diplomacy. It is well written, well documented, and objective. Its publication is indeed timely. America has a great deal, economically as well as strategically, at stake in the whole area, and one hopes that it is not too late for American political leadership to learn lessons from the mistakes of the past in its attempt to deal with the future.

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## AFRICA

JOHN MERCER. *The Canary Islanders: Their Prehistory, Conquest, and Survival*. London: Rex Collings; distributed by Barnes and Noble Books, Totowa, N.J. 1980. Pp. xvii, 285. \$32.50.

John Mercer's *Canary Islanders* focuses on the pre-European aboriginal inhabitants of the Canaries in a book that draws together all the archaeological, anthropological, and ethnographic evidence concerning them. Mercer also supplies a historical section dealing with the conquest and with post-conquest conditions, containing materials drawn from standard chronicles. The closing eighteen pages discuss the "limited survival" of the Canarians in later days, followed by a useful eleven-page appendix describing the complications of present-day Canarian politics.

There is enough information here about the aborigines to satisfy the curiosity of most people, since the picture that emerges is a disappointing one of barley-cultivating, goat-tending, cave-dwelling, skin-clad, stone-implement primitives, with a material culture that seems to fall short of that of Amazonian forest Indians. The lack of boat-building and sailing skills is astonishing, for the islanders had plenty of timber. They lived off a precooked flour (*gofio*) that could be eaten cold, made by toasting the grains before grinding. They fattened their brides to a condition of obesity. Their warriors could hurl stones in combat with unusual skill, but did not use the bow and arrow. One island (Gomera) had a strange whistled language. Such were the Canarian characteristics of more than ordinary interest.

There were two physical types (Cromagnoid and Mediterranean) on the seven islands, although both seem to have been of Berber origin. Mercer notes that Juba II of Mauretania (died ca. A.D. 20) visited the Canaries and found them uninhabited. Mercer

speculates that the Romans later used the islands as penal colonies for rebellious mountain Berbers. This ingenious theory, although not provable at present, fits radiocarbon dates for the earliest skeletal remains and would account for the lack of boating skills among the Canarians, who had to be transported to the islands by someone else.

When examining archaeological evidence, Mercer is detached and unbiased; but when writing history he turns into a passionate participant, reciting a caustic tale of "good guys" and "bad guys." He is alert to every nuance of Canarian suffering but oblivious to the plight of the Spanish peasant soldier, forcibly dragged into a dangerous and unprofitable struggle. Mercer treats fifteenth-century Spanish religious practices in sarcastic terms, although Canarian sacred sites command his appreciative attention. Some scholars have yet to learn that the canons of objective and unprejudiced social science apply to the study of all societies, not just the primitive ones.

Moreover it is not clear that the real Canarian struggle was an affair of campaigns and battles. After one thousand years of isolation the Canarians would have been highly vulnerable to an invasion of foreign micropathogens. Urban European childhood diseases would have been adult epidemics among the Canarians. Smallpox may have devastated whole communities. Large numbers of deaths could have accounted for the suicidal melancholia of the islanders during the conquest.

The 240-item bibliography, dominated by articles from five periodicals, proves the poverty of Canarian historiography and unfortunately was compiled too early to include the best American contribution, George F. Steckley's elegant essay on the "Wine Economy of Tenerife in the Seventeenth Century" (*Economic History Review*, 33 [1980]: 335–50).

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R. S. O'FAHEY. *State and Society in Dār Fūr*. New York: St. Martin's Press. 1980. Pp. xii, 210. \$30.00.

R. S. O'Fahey's important new book, *State and Society in Dār Fūr*, builds on the foundations laid in his earlier work: "The Growth and Development of the Keira Sultanate of Dār Fūr" (University of London Ph.D., 1972); *Kingdoms of the Sudan* with J. L. Spaulding (1974); and a large number of specialized articles on the history of Dār Fūr and the sources for its study. The work under review is not a political history but "a study of several interrelated themes and institutions . . . centred on the political and administrative evolution of the state" (p. vii). Through chapters on "Sultan and Fashir," "Hier-

archies and Elites," "Land and Privilege," "Sultan and Subject," "Government and Community," "Islam, State and Society," and "Raiding and Trading," the book therefore sets out to describe the evolution of the Fur state and the development of its ruling institutions.

At the center of the state were the sultan and the *fāshir*, or palace complex, institutions that the author sees as "animated by two parallel, complementary and contradictory ideologies," the Fur (or African) and the Islamic (p. 14), which informed much of the state's political evolution. At least in part because of the tension resulting from this dual identity, the Fur polity was not the ossified anachronism it was thought to be by the British officials of the Condominium regime that supplanted it. Thus, to cite one of many examples, the reversal, before the destruction of the first sultanate in 1874, of a centralizing tendency helped to bring about a weakening of the tribal system and to facilitate Islamization (p. 68).

The tension between the "African" and the "Islamic" may be detected in both the style and the substance of this book. O'Fahey's descriptions of the court and its panoply, of the functions and titles of the hierarchy, are evocative, while their development is minutely and carefully documented; his analysis of institutions goes only as far as the sources will allow, but it is invigorated by well-chosen examples and case histories (for instance the careers of Kurra and Ādam Bōsh on pages 40–42).

The most impressive aspect of *State and Society in Dār Fūr* is O'Fahey's use of sources. His mastery of them must owe much to the fact that many of them have come to light through his own efforts. In the excellent "Sources and Bibliography" (pp. 185–97) he stresses that the hundreds of interviews he has conducted and documents he has located and photographed are only a small part of the potential oral and documentary sources. This book should serve not only as evidence of what can be achieved by dedicated scholarship but also as a starting point for the further research it should stimulate. The book is indexed and includes two useful glossaries (of titles and offices, and of other terms) and three maps.

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NICOLA SWAINSON. *The Development of Corporate Capitalism in Kenya, 1918–1977*. London: Heinemann; distributed by University of California Press, Berkeley and Los Angeles. 1980. Pp. xiv, 306. Cloth \$20.00, paper \$8.50.

*The Development of Corporate Capitalism in Kenya* represents an important new direction in Kenyan economic history: the analysis of the role of foreign

capital in Kenya and its changing relations with both the state and with settler (and later, African) capital. The book is divided chronologically into three sections: the colonial period before 1945, the period between 1945 and independence in 1963, and the postcolonial period up to 1977. For each section, Nicola Swainson outlines her general argument about the directions of Kenyan capitalism and then illustrates her points through three specific case studies. For the period before 1945, for example, Swainson shows how the increasingly competitive international market influenced foreign capital to move beyond export-import trade in Kenya into more direct attempts to control production and processing of primary products. Her general points are documented through discussion of the experiences of the Bauman trading company, Magadi soda, and Brooke Bond tea.

In her discussion of the postwar economy, Swainson emphasizes increasing state intervention and the move of merchant capital into production. Of the three case studies in this section, I was particularly impressed by the story of pineapple production in Kenya and the ways in which the government, originally staunch supporters of pineapple as a smallholder crop, eventually cooperated with Delmonte's efforts to phase out smallholder production in favor of larger-scale, more controlled estate production.

The third section, on the postcolonial economy, emphasizes the role of African capital, and it is here that Swainson could make her most important theoretical contribution. Dependency theorists have long seen African capitalists as an "auxiliary bourgeoisie," holding political power in their own countries, but economically the puppets of foreign capital. The most familiar application of this theory to the history of postcolonial Kenya is probably Colin Leys's *Underdevelopment in Kenya: The Political Economy of Neo-Colonialism, 1964–1971* (1974). Swainson herself emphasizes the formation of a "national bourgeoisie" in Kenya, an independent class of indigenous capitalists whose interests do not always coincide with those of foreign capital. The contemporary importance of this class is clear, but I think Swainson is at her weakest when tracing its historical evolution. Her analysis is based on minimal research, and many questions are left unanswered. Swainson notes, for example, that the 1970s witnessed a well-defined shift of indigenous capital from trade into agricultural and industrial production, but she goes on to explain that "the forms of agrarian accumulation have been covered elsewhere, so the focus of this study will be on the expansion of enterprises in the commercial and industrial sectors" (p. 186). This was a particularly unfortunate decision, since I suspect there are important differences between the forms and the sig-



nificance of African capitalist accumulation in agriculture and in industry.

There are two methodological points that I think would have strengthened this account. First, I do not believe it is possible to construct a satisfactory analysis of indigenous capitalism on the basis of macro-level research. To demonstrate the emergence of a class of indigenous capitalists, or to argue that there was in fact any marked continuity between the influential men of the late nineteenth century, the wealthy farmers or traders of the colonial period, and the politicians and businessmen of the contemporary period—and such a continuity is crucial for Swainson's argument—detailed research at the local and district levels is required. Second, the anonymity of this account is a disappointment; the history of capital without the capitalists makes for flat reading, although it may be politically expedient.

With the above reservations, I would argue that *The Development of Corporate Capitalism in Kenya* is a useful contribution to Kenyan economic history. Both the detailed information on foreign corporations in Kenya and Swainson's emphasis on the role of indigenous capital will no doubt be incorporated in later syntheses of East African economic history.

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## ASIA AND THE EAST

LAURENCE A. SCHNEIDER. *A Madman of Ch'u: The Chinese Myth of Loyalty and Dissent*. Berkeley and Los Angeles: University of California Press, for the Center for Chinese Studies, Berkeley. 1980. Pp. x, 270. \$17.50.

Laurence A. Schneider's *A Madman of Ch'u* adds a new dimension to our understanding of Chinese civilization. Through a discussion of the ancient official and poet, Ch'ü Yüan, it demonstrates that China, like the West, had a tradition of political dissent, loyal opposition, nonconformity, and a quest for individualism. The tradition might not be as strong as its Western counterpart and has evolved in a different context, but it has existed from China's early history down to the present. Whereas the Confucian lore suggests reason and restraint, the Ch'ü Yüan lore suggests passion and unwillingness to compromise.

Ch'ü Yüan, who lived from 338 to 278 B.C., was a minister in the southern kingdom of Ch'u until he was banished from the court due to rumors spread by a rival faction. In exile, he wrote poetry to emphasize his loyalty to the king and protest his unfair treatment. His poem "Encountering Sorrow" ex-

presses his resolve to sacrifice his life in the hope of awakening his age to the truth. It conveys a feeling of individualism, independence, and isolation. The myth emerges of an official who loyally served his ruler but passionately and uncompromisingly acted on his convictions and resisted authority when it deviated from the ideals to which he was committed. The myth also reflects the link between literature and politics. An official used literature to express political dissent. Moreover, the south became associated with eccentricity and excessiveness.

This myth has inspired Chinese dissidents from the late Han dynasty on. It evolved through time and absorbed the particular features of each epoch. In classical times, Ch'ü Yüan represented the official who offered advice to the ruler at the risk of his position and even his life. His actions reflected the tension between loyalty to the ruler and loyalty to the best interests of society. In the postclassical period of the Sung and Ming dynasties and the dominance of neo-Confucianism, the conflict between ruler and official turned inward. Ch'ü Yüan was celebrated not because of his political battles but because he represented the self-cultivation that overcomes inner corruption. The Ch'ü Yüan myth gives Confucian acts a moral passion missing from the typical Confucian emphasis on moderation.

In the early decades of the twentieth century, when Western culture was inundating intellectual circles, Ch'ü Yüan took on the features of a Western hero. He was a symbol of artistic genius, a moral superman, and a martyred prophet. He became an atomistic, liberated individual. He also assumed a populist caste. Although he had been an upper-class aristocrat, he was described as a people's poet who wrote folk literature. In the People's Republic of China, Ch'ü Yüan has lost some of his autonomy as his interpreters combine his individualism and romanticism with Marxist historicism and class consciousness.

To demonstrate the power of a myth over the course of most of Chinese history, Schneider uses history, literature, and anthropology and analyzes folklore, legends, and rituals. Given his wide-ranging, interdisciplinary approach, it is not surprising that at times his exposition is confusing and at other times repetitious. Nevertheless, Schneider ably measures up to the formidable task he set for himself. The result is that he revises some of our conventional views. He convincingly shows that a myth that has captured the imagination of so many generations of Chinese must reflect an integral part of Chinese culture—a culture that evokes chaos as well as order, excess as well as balance, and individuality as well as collectivity.

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CHO-YUN HSU. *Han Agriculture: The Formation of Early Chinese Agrarian Economy (206 B.C.-A.D. 220)*. Edited by JACK L. DULL. (Han Dynasty China, number 2.) Seattle: University of Washington Press. 1980. Pp. xxv, 377. \$20.00.

This volume is a welcome contribution to the Western-language literature on premodern economic history. This welcome stems from a number of factors.

First, the economic history of traditional China is, in the West, a woefully underdeveloped field. It lags well behind the fields of intellectual, social, and administrative history. An exposition here of the reasons for this would consume too much space; the present volume clearly indicates that this need not be the case. Cho-yun Hsu's study combines a synthesis of the existing literature in Chinese and Japanese, a judicious rereading of the available textual sources, and an application of the wealth of evidence provided by the explosion in archaeological activity in China in recent decades. It includes some comparative considerations by implication. The use of archaeological data is the main strength of the study, for without it our comprehension of the nature and development of early Chinese agriculture (and, indeed, many other elements of early Chinese culture) would be far less satisfactory.

The work under review is also welcome in its context as part of the fledgling but promising series on the Han dynasty being offered by its publisher. The first volume, T'ung-tsu Ch'ü's *Han Social Structure*, had its defects. One of its advantages, however, was the incorporation of a sizable selection of translated primary source material. This pattern has been preserved in the present volume and, hopefully, will persist throughout the series. The practice adds a valuable dimension to the series by providing the nonspecialist and beginning student of China with useful sources in an easily accessible form. This is especially so since the translations are well annotated. It is to be hoped that volumes on Han government and intellectual history will follow soon in this series.

Despite its title and subtitle, Hsu's work also includes important information on the late Chou developments leading to the Han circumstances and on elements of commercial and social history that impinge closely on the story of agriculture. (Some of the discussion of social factors is a useful supplement to volume 1 of the series.) The prospective reader will find the contents of the work aptly summarized in the editor's preface, the author's epilogue, and the conclusions to each chapter. The chapters cover "Government Responses to Population and Land Tenure Problems," "Land as Wealth," "The Farmer's Livelihood," "Farming Resources," "Farming Methods and Techniques," and "The Nonagricultural Alternatives."

There are a few elements of this study that may elicit some discussion, debate, and further study. Two of these are central to the arguments of Hsu's study, the third is peripheral in this context. Did the Han stifle commercial and industrial development? (The answer hinges as much on the true extent of pre-Han development in this sector of the economy—a difficult point to establish—as on the real effects of Han anticommercial policies.) How much faith can we place in the calculations (especially in chapter 3) designed to determine the living conditions of the Han peasant? (With the nature of the sources available, the author is to be commended on his attempts, but how close to the actual conditions do they bring us?) Finally, were there "clans" in Han times? (See pages 60–61.) The answer to this question depends on one's definition, and none is offered here.

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HANS BIELENSTEIN. *The Bureaucracy of Han Times*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1980. Pp. x, 262. \$37.50.

In his preface Hans Bielenstein describes his work as an attempt to present "a comprehensive study of the Han official system" (p. vii). With certain limited exceptions—the details of border defence and the official titles peculiar to the civil wars at the beginnings of the Former and the Latter Han—he has sought to provide some description of each Han office. In seeking such comprehensiveness in a book that runs to less than 160 pages of text, Bielenstein necessarily limits himself to very brief descriptions of each office. Of this 160 pages almost half (chapter 2) is devoted to an office-by-office description of the central administration. The chapter on local administration is, understandably, considerably shorter. There are very brief chapters on the army, official salaries, civil service recruitment, power in government, and the sources used. Into this brief compass, Bielenstein has put comments on over one thousand different offices. The result is a book that is comprehensive without necessarily being thorough on any given subject. The extremely important post of chancellor, for example, is described in less than a page and a half.

This description of Bielenstein's work should not be mistaken for criticism. He did not set out to write a magnum opus thoroughly covering any one facet of Han government but rather to produce a guidebook that would provide interested students with a modicum of information on nearly all the multitude of offices of the Han government. In this he has succeeded admirably. The book will no

doubt be used as a handy reference tool by generations of appreciative students. Its utility in this regard is increased by an appendix listing over one thousand Han titles with their translations. This appendix does not give page references to the book, but these can be found in the index under the English translation of the title in question. Moreover, given the paucity of recent writings available in English, the book can serve as an introductory essay on Han administration. This is perhaps not true of the long chapter on the central administration, which presents such a mass of descriptive information that the analytical comments it contains are difficult to sift out, but the shorter, more analytical chapters present a lucid portrait of the essentials of Han government.

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PETER BUCK. *American Science and Modern China, 1876-1936*. New York: Cambridge University Press. 1980. Pp. ix, 283. \$24.95.

How do ideas of science born and developed in one environment work themselves out in practice when transferred to a radically different one? A potentially fascinating example of this process is China's introduction to Western science through the intermediary of the nineteenth-century medical missionaries, twentieth-century corporate philanthropists, and American-educated Chinese scientists examined in this book. Peter Buck's conclusion is that American science was largely irrelevant in republican China. The most general lesson to be learned from this irrelevance, he says, is that science is not value free, nor is it a *deus ex machina* that can be applied to transform society according to pre-established rational principles. The nineteenth-century science that the Chinese learned from American medical missionaries was conceptualized as a kind of natural theology, expected to support the social and moral leadership of the unspecialized educated gentlemen who mastered it. The twentieth-century Western ideal of pure laboratory science that replaced the earlier view was part of the self-definition of a powerful new professional elite. It also suggested the social myth of a form of "pure" knowledge able to dissolve the social tensions generated by America's maturing industrial system of production. When the Chinese bought this latter ideal, their infant scientific elite was condemned to a spurious cosmopolitanism that cloaked dependence on the West and practical impotence to transform a backward agrarian society.

These conclusions are certainly plausible. In dealing with the concrete substance of his cases, how-

ever, Buck's analysis constantly slips into a central incoherence. On the one hand, "American" science, studied at institutions like Cornell or planted in China by the Rockefeller Foundation, is seen as irrelevant to Chinese conditions or as interpreted by the Chinese in ways radically different from its origins. Yet, on the other hand, it is "American" science that fails in China. At one and the same time the Chinese are presented as passive recipients of a foreign model and as rejecting an alien implant.

Such contradictions might have been eased had Buck shown a more nuanced grasp of the Chinese, as opposed to the American, environment for science. In dealing with the Chinese themselves, however, he focuses solely on the "Science Society," an umbrella organization of returned students from abroad formed in 1915, which was a mouthpiece for "science" in contemporary debates on the future of Chinese culture and a lobby for institutional support for specific sciences. Buck largely ignores the earlier generation of late Ch'ing reformers who introduced ideas of science derived from nineteenth-century Western missionaries, and the possibility that early republican scientists may have formed some of their views on the relationship of science and society from this body of influential reformist opinion is not discussed. Buck fails to underscore the sharply transitional nature of the "Science Society," which by the mid-1930s had lost leadership to the specialized institutes of the Nationalist-sponsored Academia Sinica. Finally, he mistakes "Science Society" rhetoric for its institutional practice, where scientists in fact eschewed theoretical for applied research and teaching, and cosmopolitanism for dependence on the Chinese government. The radical critique of such science is that it fosters elite, urban-oriented modernization; the irrelevance of such an approach is being questioned again today in China.

As a historian of science Buck brings to his subject a perspective that he hopes will correct the deficiencies of the historians responsible for most previous work on modern science in China. Such historians have indeed suffered from inadequate technical backgrounds and naiveté about issues in the philosophy of science. Buck also takes them to task for a bias toward the culturalist view that "Confucian tradition" accounts for all the special characteristics of Chinese interpretations of Western thought. His own explanatory framework, however, simply appeals to well-worn themes of political breakdown. The best historians of Chinese thought have shown a subtle appreciation for the interplay between Chinese predispositions to be influenced and the distinctive interpretations they gave foreign ideas. In so doing they have indeed tended toward a historicism that reinforces the impression of China's "uniqueness" that Buck condemns. Yet his alterna-

tive view that those attuned to do so will recognize American resonances in "Chinese" ideas suffers from unexamined shifts in his criteria for delineating a context of meaning.

Given how desirable it is to have more historians do comparative history, it is too bad to have to point to yet another attempt that specialists on each side of the coin observed will see as half flawed. Buck's defense is that his book is primarily about American science and that cross-cultural analysis in any case teaches us largely about ourselves. The problem with this is that it vitiates the rationale for undertaking comparative analysis in the first place.

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PAUL U. UNSCHULD. *Medizin in China: Eine Ideengeschichte*. Munich: Verlag C. H. Beck. 1980. Pp. 335. DM 38.

Paul U. Unschuld, a Sinologist with training in sociology and the history of pharmacy, has given us a radically new and thought-provoking panorama of Chinese medicine extending from the Shang dynasty to the twentieth century. The traditional approach has been to search for the scientific bases of Chinese healing and lament its shortcomings when compared with Western developments (citing lack of systematic human dissections, pathological anatomy, and advanced surgery, for example). More recent studies, prompted especially by the astounding effects of acupuncture in pain control, have tried to stress the advantages of ancient Chinese medicine and demonstrate its apparent superiority over scientific curing.

Instead, Unschuld attempts to examine Chinese ideas and concepts of disease within the socioeconomic and political context of the times. His thesis is that throughout history human societies confronted with disabilities advanced three paradigms of causality for illness, namely specific nonhuman agents, magical correspondences, and natural phenomena. According to the author, such a paradigmatic "hard" core is historically supplemented by changing "soft" coats that result from specific changes in the social setting. Thus, to understand China's shifting medical theory and practice one should relinquish simplistic models of a relentless scientific progress and direct the analysis to sociopolitical ideologies and power relationships operating in each historical period.

Following such methodological prescriptions, Unschuld first examines the medical ideas of the Shang dynasty (ca. 1800 B.C.). He concludes that the Chinese society of that period strongly believed in the power of their ancestors to guide and dis-

cipline their members, including the infliction of illness as punishment for transgression of social taboos. This so-called "ancestral healing" is contrasted with a more developed "demonological medicine" said to have prevailed during the subsequent Chou period (after 1200 B.C.). Under the impact of political chaos and social breakdown, Unschuld sees the emergence of an "all against all" atmosphere in which sickness causality was in consonance with the prevailing paranoia and consisted of unexpected attacks by evil demons.

Later, in the third century B.C., the author proclaims the emergence of another "medicine of systematic correspondences," rooted in Confucian ideology and imperial integration. The book proceeds by examining Taoism and the development of Chinese *materia medica*, Buddhism and Hindu healing, and Sung neo-Confucianism and medical thought and ends with chapters on the Ming and Ch'ing dynasties and contemporary efforts to blend traditional Chinese and Western medical concepts.

For readers unacquainted with the German language, a summary of Unschuld's ideas is available in two recent articles: "The Development of Medical-Pharmaceutical Thought in China" (*Comparative Medicine East and West*, 5 [1977]: 109-15, 211-31) and "Concepts of Illness in Ancient China: The Case of Demonological Medicine" (*Journal of Medicine and Philosophy*, 5 [1980]: 117-32). He has offered us a valuable tool for the re-examination of relationships among sociopolitical events, value systems, and the conceptualization of sickness necessary for an understanding of medical care. Such scaffolding could be fruitful in the examination of Western healing systems as well.

At the same time, Unschuld adheres much too closely to a social determinism of medical ideas. Nothing is said about how society forms a concept and reacts to the impact of epidemic disease, and how such ideas influence medical theory and practice. His paradigms of illness causality are also quite arbitrary and subject to challenge. Further analysis of the origins of empirical drug therapy—largely undeveloped in the book—and integration of recent archaeological discoveries and scholarship will undoubtedly result in a panorama of greater medical diversity than Unschuld has portrayed.

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ELIZABETH J. PERRY. *Rebels and Revolutionaries in North China, 1845-1945*. Stanford: Stanford University Press. 1980. Pp. xiii, 324. \$25.00.

In this very fine monograph, Elizabeth J. Perry analyzes the interaction of environment with social or-



ganization in three historical contexts: the Nien uprising of the mid-nineteenth century, the Red Spears rebellion of the early twentieth century, and the Chinese communist movement in Huai-pei from the 1920s to 1945.

The Huai-pei region is situated on the southern edge of the north China plain. Its harsh environment, reflected in frequent local droughts and floods, nurtured aggressive survival strategies among rural residents: the predatory strategy, embodied in organized smuggling and banditry, and the protective response to predatory threats. Predation was a form of collective action organized along lineage lines. Protective strategies were also organized collectively, but by communities. Although the interests of predators and protectors seem to be fundamentally opposed, these groups occasionally united against external threat.

Ecological disaster or government action might stimulate mobilization of bandit gangs or local militia into larger movements. The stimuli for the Nien rebellion were the floods and earthquake of 1852, which produced bandit alliances with fortified communities in opposition to the government. In republican times, attempts of warlords and the Koumintang government to penetrate local communities and raise taxes stirred the Red Spears, a rural self-defense movement, into protective rebellion. Both movements exhibited the tenacity of local institutions as well as their inability to organize into larger movements with goals transcending the survival strategies born of the Huai-pei milieu. As long as the basic conditions of life in the region remained unaltered, these institutions were bound to persist.

When the communists came to Huai-pei, they tried first to organize rural revolution through the Red Spears and, when that failed, through bandit gangs. The war with Japan altered politics: communist land reform programs were pushed to the background, and protective and predatory groups were enlisted in a national resistance movement. By 1944, the Communist party's strengthened position in Huai-pei enabled it to shift to a new policy of mass mobilization, which also tried to alter the basic uncertainties that had promoted traditional survival strategies and institutions. As the environment improved, so did the possibility of transforming peasant behavior in Huai-pei. Yet, as Perry notes in a concluding chapter, there is evidence in the period after 1949 that in Huai-pei man and nature were not easily tamed.

This is not only a provocative study of the ways in which an environment can foster social mobilization but also a skillful synthesis of historical and social science analyses. Its inclusion of movements in the late Ch'ing and republican periods produces rich insights that demonstrate the importance of

historical research that cuts across the usual periodization. Perry's study fits into regional systems analysis and points to the need for work on other peripheral zones as loci of traditional rural unrest.

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STEPHEN R. MACKINNON. *Power and Politics in Late Imperial China: Yuan Shi-kai in Beijing and Tianjin, 1901-1908*. (Center for Chinese Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1980. Pp. xii, 260. \$18.50.

Stephen R. MacKinnon's modest volume on Yuan Shi-kai's rise to prominence in the last decade of Qing rule is a significant contribution to two strands of revisionist history. It follows the path broken by Ernest P. Young's superb study of the *Presidency of Yuan Shih-k'ai* (1977) in casting our image of Yuan less as an ambitious, conspiring militarist than as a centralizing, bureaucratic reformer. In addition, the present study supports the argument most associated with MacKinnon's mentor, K. C. Liu, that the late Qing was not undergoing a progressive regionalization due to dynastic decline but was in fact showing a continuing and finally increasing centralization of power in Beijing.

MacKinnon's specific concern is Yuan Shi-kai's years as Zhili governor general (1901-07) and grand councilor and foreign minister in Beijing (1907-08). He attributes Yuan's rise to three key resources: foreign (especially British) support, military power, and judiciously cultivated allies at court. He carefully distinguishes Yuan's Beiyang army from the mid-nineteenth-century armies recruited through personal and regional networks and stresses its financial dependence on the central government. The chapter on Yuan's remarkably successful educational, police, and economic reforms is certainly the best and most important—arguing persuasively for a high degree of cooperation between magistrates and local elites in Zhili. MacKinnon further maintains that when Yuan was removed from Zhili, he was able to "enhance his political power" (p. 182) in his final sixteen months as grand councilor in Beijing, before his forced retirement in January 1909.

Throughout, MacKinnon's analysis proceeds in terms of "three simultaneously expanding and overlapping nodes of power" (p. 10): the central government, provincial governments and armies, and local elites. (A complete model should probably add a fourth node, imperialism, whose regular interference in Chinese domestic politics MacKinnon, like Young, effectively documents.) His conceptualization is an important reminder that power was

not a zero-sum game and that when actors at each node cooperated rather than competed, each could expand its power. It still remains to investigate precisely how these nodes of power interacted in the late Qing. How much of Yuan's ascent to power at the center was merely a reflection of his achievements in the provinces or of his clique's continuing control of provincial power in north China? If the center was as powerful as MacKinnon and Liu stress, why did Yuan's men appear so overwhelmingly in northern posts (pp. 198-99)?

Finally, MacKinnon's study raises some interesting questions with respect to regional differences and the 1911 revolution. We have usually attributed the weakness of the revolution in the north to the strength and loyalty of the Beiyang army. Yet MacKinnon, in discussing the late Qing reforms, contrasts cooperation between magistrates and local elites in Zhili to competition between the two "nodes" in central China. Were cooperation and the relative success of the Zhili reforms also factors that weakened revolutionary forces? Or are the success and cooperation that MacKinnon sees mainly the result of his study's terminal date of 1908, before the local self-government assemblies gave the local elite the independent institutional bases from which to challenge the authority of the magistrates? Whatever the answers, MacKinnon has given us a solid study that will allow scholars to proceed to the next stage of inquiry into state and society in late Qing north China.

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JÜRGEN DOMES. *Socialism in the Chinese Countryside: Rural Societal Policies in the People's Republic of China, 1949-1979*. Translated by MARGITTA WENDLING. London: C. Hurst or McGill-Queen's University Press, Montreal. 1980. Pp. ix, 189. \$30.95.

As the result of several factors such as United States involvement in the Chinese civil war, the McCarthy "Who Lost China" era, the Korean conflict, and Vietnam, the field of the study of the People's Republic of China (PRC) is marked by extreme dichotomy. Jürgen Domes, in reaction to this phenomenon, has given himself the task in his publications of trying to find the middle ground between what he describes as the unthinking anti-communist approach and the unthinking procommunist approach.

In this particular book Domes attempts to both narrate and present an unbiased evaluation of the social and economic performance of China's rural policies during the last three decades—all this in the space, excluding appendixes, of little more than one hundred pages.

This is a formidable enterprise, and the more skeptical reader might well raise a suspicious eyebrow or two at the outset. Unfortunately, the suspicions prove well founded. Domes's account of the stages of China's agrarian transformation—land reform, collectivization, communization, revision, and the post-Cultural Revolution era—is, at best, cursory. In his evaluation this superficiality is not only continued but compounded by conclusions that lack the objectivity he advocates.

His generally negative judgment of socialism in the Chinese countryside is based on rather questionable grounds. First, he dismisses out of hand the overwhelmingly positive reports of the numerous outsiders who have visited China. His rationale for this is that all their visits were heavily orchestrated. Next, he challenges the information released by China because one "necessarily [has] serious doubts about statistical data given by PRC sources." Finally, based on a table on the development of grain and potato production, 1931-79, Domes concludes that production has not kept pace with population growth.

There are several problems with this conclusion, most of which are related to Domes's statistics. His figure of 171 million tons of grain and potato production for the 1931-37 period is highly suspect, and, unfortunately, Domes does not give his source. His figure for 1949 of 150 million tons is also questionable. Most economists cite 111 million tons. If this is accurate then the increase in grain and potato production from 1949 to 1979, when it was over 300 million tons, was certainly sufficient to keep pace with the population increase.

On the positive side, it is only with what appears to be extreme reluctance that Domes concludes that remarkable changes have taken place in the social structure of the myriad of Chinese villages. He notes a limited equalization of peasants' income; remarkable progress in health services; some progress in education; a more balanced distribution of the yield between agricultural surplus and deficit regions; and improvements in water conservancy, the use of chemical fertilizers, and the use of agricultural machinery.

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DONALD RODEN. *Schooldays in Imperial Japan: A Study in the Culture of a Student Elite*. (Center for Japanese and Korean Studies, University of California.) Berkeley and Los Angeles: University of California Press. 1980. Pp. xiii, 300. \$24.50.

In the sixty years preceding 1949, the most important schools in Japan were not the imperial universities but the elite higher schools that prepared

young men for the professional colleges that composed the universities. Most of modern Japan's political, academic, and business elite were graduates of both a higher school and an imperial university. But the "old school ties" that influenced them most profoundly were the ties to their higher school, not to their university. The chief reason was the atmosphere of the higher school campus, which Donald Roden summarizes as a unique "web" of shared "monasticism, elitism, intellectualism, and asceticism" (p. 229).

Roden's book is primarily an account of extracurricular life at the higher schools, especially Ichikō, from their creation in 1886 to their abolition in 1949. It is based on prodigious research in school magazines, "dormitory histories," alumni publications, memoirs, and even lavatory graffiti (p. 238). What emerges most clearly is the depth and intensity of the affection students and alumni felt for their higher school as their "spiritual birthplace" (p. 247) and how quickly that school spirit developed. What had taken centuries at Eton materialized almost immediately in Japan's Etons. Roden attributes this partly to a few astute headmasters but primarily to the students themselves. Dormitory residence was compulsory and cloistered, and from very early the students had the right of self-government, including almost total responsibility for discipline.

The higher school spirit was consciously elitist, politically conservative, and fundamentally patriotic and nationalistic, yet it was intellectually much more European than Japanese. Roughly half of the curriculum hours were in European languages and literature. A favorite student anthem was the "Dekanshō": "Descartes, Kant, and Schopenhauer! Half the year we live with them, the other half we sleep!" (p. 216). Japan's higher school students were more familiar with the ideas of Western philosophers than many of their Western contemporaries. The latter would have been hard pressed even to name more than one Asian philosopher.

Roden writes with grace, wit, and admirable control of his diverse materials. He has provided a cogent and scholarly analysis (enlivened by well-chosen anecdotes, student quotations, and 28 photographs) of a student ethos, rites of passage, and "fortress mentality" that until now have been only dimly and imperfectly perceptible to outsiders. It is an important book because that ethos shaped the attitudes and friendships of a majority of modern Japan's leaders after 1886.

One's only regret is that the author says so little about formal schooling. Class attendance was not compulsory (p. 227), and graduation was virtually assured regardless of grades (pp. 99, 163, 247). Yet the only classroom photograph (p. 218) shows a packed room and a blackboard covered with writ-

ing in four languages. Roden found that extracurricular activities became increasingly intellectual as time passed, including even "a new breed of philosophically inclined athletes" (p. 178). And the students were impressively (indeed precociously) articulate in extracurricular writing and speaking. Since the higher school (not the university) was the highest level of general education, the extracurriculum must have derived much from the curriculum.

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EDWARD J. DREA. *The 1942 Japanese General Election: Political Mobilization in Wartime Japan*. (International Studies, East Asian Series Research Publication, number 11.) Lawrence: Center for East Asian Studies, University of Kansas; distributed by Paragon Book Gallery, New York. 1979. Pp. 223. \$8.00.

Perhaps more than any other national emergency, wartime tests the strength of parliamentary government and other constitutional processes; this is true above all in a total conflict like World War II. This thoroughly researched monograph by Edward J. Drea, a historian at the U.S. Army installation at Fort Leavenworth, Kansas, reveals how extensively Japan preserved constitutional rule at the height of its mobilization for war.

Japan's Meiji Constitution of 1889 was among the half-dozen earliest national constitutions in the world. It survived both depression and war until the American occupation authorities radically amended it in 1947 with a text that is still in effect today. Drea's study analyzes the signal wartime challenge to parliamentarianism under this document—the effort of the Tōjō government to rig the 1942 general election.

The imperial diet, which first convened in 1890, consisted of an appointed House of Peers and an elected House of Representatives (all adult males voted since 1928). The cabinet was theoretically appointed by the emperor and responsible to him alone, but in practice it was dominated by party politicians from the lower house during most of the period from 1918 to 1931. After 1936, former military officers began to monopolize cabinet positions. Four years later, political parties were nominally merged into a single organization, the Imperial Rule Assistance Association, to unite the country for war.

But the lower house remained surprisingly critical of government policies even after Pearl Harbor. Early in 1942 Prime Minister Hideki Tōjō and certain IRAA leaders created a council to recommend an "approved" candidate for each of the 466 lower house seats in the forthcoming April 30 election. In almost tedious detail, Drea portrays the recommenders, their candidates, and the many critics of

this unprecedented meddling. The Home Ministry suppressed many "dangerous ideas" during the campaign, but neither its police units nor the military had much impact on the choice of candidates. Ironically, many former party politicians were recommended because the council wanted likely winners.

Although 381 approved candidates won, the election produced only slightly more turnover in the lower house than in 1928 and yielded a diet scarcely less critical of the government than its immediate predecessor. The author ably explains how constitutional procedures set firm limits on the cabinet's effort to hush its parliamentary critics.

Unfortunately this thorough and persuasive study does not treat the larger question of competition among the wartime military, industrial, bureaucratic, and palace elites. The text reveals inadequate editorial care: romanizations of Japanese terms are inconsistent and the prose style is needlessly awkward throughout. These defects cannot mask the solid achievement of Drea's book.

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GREG DENING. *Islands and Beaches: Discourse on a Silent Land—Marquesas, 1774–1880*. Honolulu: University Press of Hawaii. 1980. Pp. xii, 355. \$27.50.

This book is confusing, frustrating, and at times innovative. It is a study of the Marquesas Islands and their people in the one hundred years after contact with Western culture in 1774. Three strands are interwoven: a history of the people and the islands; an anthropological investigation into the culture of the people; and a discussion of the theories, methods, and methodology of many social anthropologists, mostly in relation to the Marquesans. To this mixture are added Greg Denning's personal philosophical thoughts, in the form of a so-called "Reflection" at the end of each chapter except the final one. The result is a mélange of facts, philosophy, and fancy, written in an unclear, stilted prose and made more difficult for the ill-prepared reader by the use of Marquesan words for the islands themselves, the Marquesans, Westerners, and other terms.

Denning has displayed a great deal of erudition in treating a difficult subject, as one can see by his detailed and excellent bibliography. It is unfortunate, however, that he does not cite sources in the text, making it difficult for the readers to know if they should disagree with him or with his sources.

The Marquesas have been studied less than either the Hawaiian or the Society Islands, with good reason besides their comparatively remote location: there are fewer historical sources because the Marquesans were so prone to violence among themselves and against visitors. An explanation con-

cerning Marquesan violence forms a large part of Denning's book; it is interpreted both historically, as an integral part of culture, and theoretically. This is useful but confusing and raises the question as to the intended audience of the book. For this historian, the book's use is severely limited, which is all the more unfortunate because of the possibilities inherent in the subject matter and the author's obvious wealth of material.

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## UNITED STATES

WILLIAM APPLEMAN WILLIAMS. *Empire as a Way of Life: An Essay on the Causes and Character of America's Present Predicament Along With a Few Thoughts About an Alternative*. New York: Oxford University Press. 1980. Pp. xiv, 226. \$14.95.

Some of William Appleman Williams's readers have long felt that he is too much a polemicist to be a good historian. This book will abundantly confirm their opinion. In it Williams sets out to attack present-day militarism as the product of an American drive for empire, first over Indians and blacks, then over Mexicans, and finally over the whole world excepting the USSR and Eastern Europe. Imperialism, he argues, has been the main force in American history.

This simplistic thesis produces strange and wonderful judgments. The Revolutionary War becomes a struggle for succession to a British empire. James Madison provides in the Constitution the centralized power that alone will make empire possible. The War of 1812 results mainly from Western territorial ambitions and Eastern desire for markets. The Civil War is an "imperial war for freedom" (p. 91) in which Abraham Lincoln makes "a bargain with the Devil" (p. 92) to crush Southern culture and save the Mississippi valley for capitalism. The Spanish-American War, a "gratuitous attack on Spain" (p. 112), is motivated by American desire for the Philippines. American actions at the Portsmouth and Algeciras conferences exemplify essentially the same view of American police power as the Roosevelt corollary to the Monroe Doctrine. Woodrow Wilson leads the country into World War I to deploy that police power further and "usher in a millennium of democratic progress" (p. 134) based on American capitalism. After 1932 Williams's rhetoric grows more intemperate. Franklin D. Roosevelt advances the American empire by saving capitalism, increasing armaments spending, consolidating executive power, and finally "[committing] the government to a war for America's imperial way of life" (p. 164). Eager to win the war



fast with minimal disturbance to American social institutions, he cynically puts off black demands for equality and Russian demands for a second front. Harry Truman continues Roosevelt's imperialism virtually unchanged, and after a brief interlude of realism under Dwight D. Eisenhower, John F. Kennedy reverts to the "psychopathology of the empire at bay" (p. 198). Lyndon B. Johnson and Richard M. Nixon come off comparatively well, and Williams ends his account with attacks on Zionism and oil diplomacy. Not surprisingly, the American captives at Tehran are "hostages to the American Empire" (p. 207).

A balanced, penetrating analysis of the role of expansionism in the American past would be a legitimate, even a long-needed addition, but Williams's exaggerations do no service to either his readers or his reputation. He repeatedly begs the question, assuming the truth of what he has set out to prove. His "proof," where it exists at all, usually consists of a few quotations, as if one man's statements and a nation's deeds were synonymous. Taken out of context, the quotations are blown up into false significance, and for lack of citations the reader cannot always check them. The intellectual basis for the imperial thesis consists largely of vaporous expositions of Adam Smith's remarks about marketplace capitalism, already familiar to Williams's readers. He attributes nearly all actions to someone's deliberate planning and ignores most of the voluminous evidence contrary to his thesis. Especially in later chapters he indulges in snide insults.

Although at the outset Williams promises an alternative to the imperialism of America's wicked past, he is so much carried away with his historical interpretation that he offers few profound and no specific suggestions: create a more humane life. Learn to walk slowly. Know thyself. Follow the Golden Rule. Don't steal. Thus Apocalypse fizzles out in clichés.

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ALLAN R. MILLETT. *Semper Fidelis: The History of the United States Marine Corps*. (Macmillan Wars of the United States.) New York: Macmillan or Collier Macmillan, London. 1980. Pp. xviii, 782. \$29.95.

*Semper Fidelis* is a carefully crafted institutional history that draws heavily upon the techniques of organizational theory. Allan R. Millett, a Marine Corps reserve officer and director of the Program in International Security and Military Affairs at the Mershon Center, Ohio State University, sees the story as one of institutional survival and adaptation.

The book hastily skips across the first century and

a quarter of Marine Corps history, which embraced naval police and landing party responsibilities. In terms of the roles played by the corps and its inability to develop a well-enunciated place within the military structure, this is about as much attention as the story deserved. In the succeeding colonial infantry period, which Millett considers stretched from the Spanish-American War nearly to World War II, the Marines undertook a police role in Latin America and China. That required them to protect American interests and to attempt to bring domestic quiet to volatile peasant states that lacked traditions of political tranquility. Happily for the corps, those unrewarding and often frustrating duties gave way during World War II to service as the nation's amphibious specialists. Its wartime role permitted the Marine Corps to develop both the organization and the popular support that carried them through the difficult initial phases of defense unification. The amphibious assignment is still accepted by the corps, although increasingly in recent years it has served as the country's premier force in readiness.

Because Millett's study, like Russell Weigley's army volume in the same series, has institutional concerns, the book pays very limited attention to operational history. One result that will disconcert some readers is that the relative space devoted to different periods is disproportionate to their usually perceived importance. Some of the disparities are striking. For instance, World War II, which saw more Marines in combat than any other conflict, is covered in fewer than 100 pages, while the Banana Wars and China occupy 117. The latter do not deserve their extensive coverage. Conversely the relatively short Korean and Vietnamese sections are incisive guides to the problems of the Marine Corps in its relations with the defense establishment.

This first volume to appear in six years in "The Macmillan Wars of the United States" series continues the reputation for quality. Well written and wide ranging, *Semper Fidelis* is clearly a product of the "New Corps" and unlike any history of the service seen before. None that follow will dare to ignore it.

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STEPHAN TERNSTROM *et al.*, editors. *Harvard Encyclopedia of American Ethnic Groups*. Cambridge: Belknap Press of Harvard University Press. 1980. Pp. xxv, 1,076. \$60.00.

The appearance of the *Harvard Encyclopedia of American Ethnic Groups* is an important publishing event. The volume contains entries for 106 groups, 29 thematic essays, and many useful maps and tables. Be-

cause it is so comprehensive, it will quickly become an invaluable reference work for those seeking information about American ethnicity.

The editors wisely decided not to define ethnic groups in rigid terms; rather they employ a loose characterization of ethnicity that permits them and their authors flexibility. Hence, one will find selections on (white) Southerners, Yankees, and Iranians. The first two are not always included in discussions of American ethnic groups, and the author of the Iranian entry is not certain whether Iranians are really a single ethnic group in the United States. Yet it is useful to have all three. The editors also had to decide which groups to include and eventually settled on the 106 here included. They note that their choice does not mean that there are only 106 ethnic groups in the United States but that there are at least that number.

The essays on individual groups vary in quality, length, and approach. They are written by scholars from different disciplines; hence some are more historical than others, but most articles try—usually with success—to blend a historical account with other approaches. In a few cases where a group is recent to the United States or information about it is lacking, the group's background receives more emphasis compared to its American experience. Generally, the essays are of high quality and informative, and, because each contains a short bibliography, they offer the reader a guide to further materials.

Those selections on groups originating in Europe and Asia are especially good and detailed. Kathleen Neils Conzer on the Germans, Humbert Nelli on the Italians, and Arthur Goren on the Jews, to mention a few of the longer pieces, pull together an immense amount of material and are excellent. Much the same can be said for groups for which prior research and writing, at least in English, has been scarce. Elliott Barkan on the French Canadians and Patrice Louis-René Higonnet on the French, for example, are quite good and original. Because of the special care given to Asians and Europeans, the volume also has shorter and valuable essays on little-known groups such as the Frisians, Kalmyks, Kurds, Assyrians, and Azerbaijanis.

Some scholars might be troubled by the editors' choices. Central and South Americans—a rather large and diverse group—are discussed together. Columbians receive extensive treatment in this section, but others from Central and South America do not. The inclusion of South and Central Americans under one heading is especially noticeable because small Asian and European groups, including those with only a few hundred or thousand people in the United States, receive separate entries.

This reviewer also found the discussion of Arabs and Middle Eastern groups in general to be trou-

blesome. To place all Arabs under one heading seems odd. Furthermore, few people know what an Arab is. The essay should have begun with a working definition or discussion of that term. The selection talks of Arab culture and the Arab world but should have been more precise. For example, there are entries under some (Eastern Orthodox, Muslims, Copts) but not all religious groups in the Arab world. Moreover, some (Assyrians and Kurds) but not all non-Arab groups in the Arab world receive separate treatment. It is not always clear what separates Arabs from non-Arabs.

The choice of placing American Indians under two long entries is understandable. To have a separate entry for each of the 170 tribes in the United States would have made the volume much longer. This decision does mean, however, that certain aspects of Indian culture and history receive less extensive treatment than do European groups. Pre-Columbian Indian history is slighted, and this reviewer also thought that American Indian family, cultural, and religious life could have been discussed more fully. The author refers to Indian religious practices or culture and even particular items, such as the Ghost Dance, but does not always explain what these are.

Many of the thematic essays are excellent. "Immigration: History of U.S. Policy" and "Naturalization and Citizenship" clearly explain the historical development of these topics. "Survey Research" appears at first to be an unusual choice, but it does give the reader a good summary of recent social science findings about ethnicity. David Ward's "Immigration: Settlement Patterns and Spatial Distribution" also contains useful information. The essays "American Identity and Americanization" and "Assimilation and Pluralism" overlap to some extent; yet each approaches its subject differently and in a complementary way.

Other thematic essays cause problems. The article on "Language Maintenance" is poorly written and difficult to understand. George Frederickson and Dale Knobel wrote a comprehensive essay on "Prejudice and Discrimination: A History of," but the one following, "Prejudice and Discrimination: Policy Against," neglects religious discrimination and concentrates on race and recent issues. "Family Patterns" is interesting but reflects the bias of the *Encyclopedia* in concentrating on European immigrants and their children. The essay on "Labor" is good, but the title is a misnomer since it deals with labor unions and not labor generally.

"Health Beliefs and Practices" illustrates the difficulty of including articles by scholars from different disciplines. Although the author notes that health practices often depend on whether individuals are immigrants or American-born and how much they have assimilated, it is not clear from the

discussion about which generation the author is speaking. As a historian, I was uneasy about some of the sweeping statements concerning the health beliefs and practices of Anglo-Americans, Hispanic Americans, and others.

Because of the limitations of space, choices had to be made about which themes to include. Some, such as "Naturalization and Citizenship," had to be included, but in other cases the choices will not please everyone. One will find entries under literature and folklore but not music. Topics like "Politics" and "Leadership" receive separate attention, but the immigrant press, theater, and various benevolent and fraternal societies do not. Labor unions are discussed but business is not. Nothing is included about refugees and repatriation. Refugees might not be a strictly ethnic topic, but under "Immigration: Social Characteristics" the contributions of immigration to production and population growth are discussed, and these are not exactly ethnic issues either. It seems to me that refugees are just as important or more so.

These criticisms highlight a basic problem with the *Encyclopedia*: the need to make choices in order to produce a one-volume book that would be useful to students, scholars, and general readers. On balance the *Encyclopedia* succeeds. The editors' own assessment that the group entries are the core of the volume is correct. Anyone wishing to learn about American ethnic groups will have to begin with the *Harvard Encyclopedia*. Its extensive coverage and many excellent essays make it an indispensable guide for students of American ethnicity. Every college, university, school, and local library, as well as many individuals, should have a copy.

DAVID M. REIMERS  
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DAVID NASAW. *Schooled to Order: A Social History of Public Schooling in the United States*. New York: Oxford University Press. 1979. Pp. xii, 303. \$13.95.

This book summarizes what has become known as "revisionist" educational history. It concentrates on three distinct periods of major change in American education: the common school era of the 1840s and 1850s, the transformation of the high school at the turn of this century, and the expansion of higher education since World War II. In each of these periods, an attempt is made to place the educational changes within a broader social and economic context. David Nasaw's contention is that in each of the periods under study schools have been expected to prepare youth for the dominant social order. Reformers, he argues, have held an equilibrium model of schooling; educational reforms would reestablish order during periods of social instability. Public

education has been shaped by an ethic of cultural homogeneity; its major functions have been to prepare an industrial labor force and experts to manage the corporations and to lay the basis for the acceptance of an inegalitarian social structure. In all this, Nasaw recognizes a persistent tension within education between democratic aspirations for egalitarian outcomes and subservience to an inegalitarian social order. While the latter has tended to dominate American education, the former was nonetheless essential to American educational history: working-class youth rejected tracking into dead-end vocational programs and pressed for access to college; women and nonwhites demanded egalitarian treatment in their schooling. These democratic pressures have forced compromises in what has otherwise been, according to Nasaw, an educational system subservient to the dominant social classes.

Nasaw tells his story well. The three periods he studies are clearly the major turning points in American education, and it is worth pointing out the continuities among them. Certainly, they are the periods best studied by historians. The writing and argument are clear. The book is limited, however, by its near exclusive reliance on existing work in the field. Monographs by Katz, Kaestle, Tyack, Lazerson, Spring, Bowles and Gintis, and others already cover much the same ground, often with more sophistication. Much of the discussion thus adds little that is new to the revisionist interpretation. *Schooled to Order* is more a summary of American educational history from a left perspective than a major contribution to original scholarship. It ought to be read, but it is to be hoped that current research will considerably extend the analysis beyond its somewhat limited confines.

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WILLIAM F. DUKER. *A Constitutional History of Habeas Corpus*. (Contributions in Legal Studies, number 13.) Westport, Conn.: Greenwood Press. 1980. Pp. 349. \$29.95.

A writ of *habeas corpus ad subjiciendum* is a common law procedure that is, simply, an order by a judge commanding a person alleged to be wrongfully detaining another to produce the latter's body at a specified time and place for a hearing on allegations of illegal detention, which if substantiated result in a granting of the writ (countries using the inquisitorial system of justice have comparable, but different, methods). Ordinarily the detainee (or someone in his behalf) petitions for the issuance of the writ and, while technically discretionary, it is

rarely denied—federal and state statutes and rules (for example, 28 U.S.C. 2241 *et seq.*), as construed, prevent an abuse of discretion. Zachariah Chafee felt this writ to be the most important human right in the Constitution, and William F. Duker mirrors this view “in the development of liberty” (p. 3).

Since Coke, Holdsworth, Maitland, Dicey, and scores of others seemingly have plumbed the English antecedents of our American writ, and there have already been studies of its constitutional history, why another? Duker’s answer is twofold: first, “heretofore no book has been published on the history of the Great Writ” (p. 7), and, second, the writ has, for us, changed markedly in application and in substance. There may be other works to refute the first claim (for example, D. J. Meador’s *Habeas Corpus and Magna Carta* [1966]), but it is the second claim, also not new, that concerns us here. Analysis must start with the negative language in Article 1, Section 9, clause 2 of the Constitution (Section 10 limits the states but has no such clause), which prevents the writ’s suspension except in rebellion or invasion. To whom does the clause apply and with what effect? For example, Congress early extended the federal process to state prisoners, in other words, a constitutional change by the legislature (although somewhat modified by the 1867 Habeas Act); however, it is the “judiciary [which] has functioned as the primary agent of constitutional change” (p. 308), although in effect flip-flopping even here. For example, in 1976 the Supreme Court held that recognition of the exclusionary rule was not required in federal habeas corpus where state courts had given the defendant a full and fair hearing (p. 264, citing *Stone v. Powell*) and that a state prisoner who failed to present a timely challenge to the composition of a jury could not, after conviction, obtain federal habeas corpus (p. 224, citing *Francis v. Henderson*). We may note that Senate efforts to overturn these decisions were unsuccessful).

How does Duker prove his major thesis of judicial constitutional change? His seven chapters open by exploring the writ’s English origins and extension to the colonies and include the disclosure that every state had provided for it by 1787. Therefore, why “was there a need to provide for a federal writ” (p. 116)? Contemporary records disclose that some of the Constitutional Convention’s participants felt the clause constituted a federal limitation on the states’ powers only during a rebellion or invasion. Others felt that it restricted only the federal government and its prisoners. Still others held that it was a grant to Congress of power to enact a habeas corpus law. Duker adopts a fourth view, “that the habeas corpus clause was designed to restrict Congressional power to suspend habeas corpus for federal prisoners” (p. 135). This conclusion on federal-state relations is reached in a somewhat legalistic-political

discussion of the early cases and consequent federal superiority, with the exhaustion doctrine as a limitation. Duker might also have referred to the political-judicial repercussions of this exercise of power by a single federal judge after the entire state court system, appellate and trial, had decided against a prisoner (the Supreme Court might also have denied review), as well as the blistering language by which the Conference of State Chief Justices condemned this intrusion. Duker’s chapter on “The Scope of Judicial Inquiry” under the writ involves a further technical analysis of the cases (and an attack on Justice Powell’s [majority] views), and the final substantive chapter on “Custody” discloses the broadening over the years from a *strictissimi* inquiry into jurisdiction into some examination of the merits. (Illustrations could have been given in other areas, for example, in bail deportation cases an action for a declaratory judgment is used in place of the writ.) In the last chapter Duker returns to his original thesis of habeas corpus change and inquires, among other questions, whether a written constitution has any significance. He concludes that “The answers to such question, however, are beyond the scope of this study” (p. 307).

The volume has a substantial bibliography of twenty pages, an index, but no table of cases or statutes. It is a good work and provides a speedy course in the English background and American adoption and use of the writ, although for non-lawyers the extended legal analyses may prove difficult reading.

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ALEC BARBROOK and CHRISTINE BOLT. *Power and Protest in American Life*. New York: St. Martin’s Press. 1980. Pp. xiv, 375. \$28.00.

This study is an earnest effort by a pair of British scholars at the University of Kent to trace “newer or underrated trends in American pressure-group activity and their roots in American history.” The subject is rich, and Alec Barbrook and Christine Bolt approach it in full realization that the degree to which American life has fragmented into interest groups is one of the hottest current concerns. The problem here is that the mass of data on group theory variants and the history of protest groups is not tightly knit. Instead, the reader gets a highly suggestive cluster of details to connect in anticipation of an important, if not necessarily pretty, picture.

At the center is a tentative thesis that is well worth testing. Barbrook and Bolt contend that the rise of pressure groups after 1800 developed from causes and along lines that have remained constant. Avowing that group theorists have overemphasized



economic factions, Barbrook and Bolt concentrate on groups that have been primarily concerned with attaining social justice—representing women, native Americans, blacks, and other ethnic groups. Their struggles, the authors conclude, have shown that attempts to reform within the system have worked far better than resort to radicalism. And, yet, during the past fifteen years the maturing success of gradualist pressure groups has merged with the residue of New Left agitation to produce a novel emphasis on the public interest. Most notably, the resultant pattern has struck a balance between consumer groups, led by Ralph Nader's "network," and political action groups, spearheaded by Common Cause.

To help give their thesis analytic roots, Barbrook and Bolt refer to Daniel Bell's judgment in his writing on postindustrial society that a communal ethos is likely to emerge as educated persons, more and more devoted to the service sector, come to have a dominant influence on public policy. Barbrook and Bolt cite with approval Edward Banfield's dictum that the new professional and influential groups are "public-regarding" rather than "private-regarding." And they conclude that "much of American life is now touched by the concept of the 'public interest.'"

That thesis, however, calls to mind a paradox, which Barbrook and Bolt suggest but never pursue, that harks back to the old debate over conflict and consensus in American history. Barbrook and Bolt note how "curious" it is that Americans should glory in their individualism and yet be commonly regarded in terms of groups. Rather than examine that paradox, however, they chart the historical evolution of groups toward a public interest stance, neglecting the persistence of individualism, which the "me-generation" displays in abundance. They also acknowledge criticisms of group theory as elitist—and congruent evidence that interest groups, including Common Cause, tend to be oligarchic—but do not deal with the implications for their concept of a "public-regarding" society.

Thus certain prime questions go begging. Are we really fashioning the communal ethic Bell foretold? The rising tide of Reagan economics suggests not. And Barbrook and Bolt themselves state that growing alarm over scarcity has undercut the environmental movement, indication that Americans prefer material abundance over democratic virtue, just as David Potter in *People of Plenty* argued they always have. Second, does the issue of public-regarding versus private-regarding indicate that the continuous core of American history has not been the group but a strain between individualism and society? Third, has the influence of groups in American life been mostly to enhance or detract from democracy? Have groups encouraged participation or

converged on a power elite? And, finally, has the proliferation of groups served to strengthen national unity by giving their members a stake in society, or is that proliferation pulling the country apart?

These unanswered questions surround a provocative study that is long on intelligently considered detail but short on coherent conclusions. Its value is as a source book and stimulus for thinking about some vital issues.

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RICHARD WHITE. *Land Use, Environment, and Social Change: The Shaping of Island County, Washington*. Seattle: University of Washington Press. 1980. Pp. xi, 234. \$12.95.

In a way typical of new disciplines, environmental history began with broad brush strokes. Frederick Jackson Turner took the entire frontier experience as his subject. Others have written about the American encounter with nature, wilderness, Indians, forests, and national parks. But as the discipline matured it produced more finely focused monographs. Books concerning a single national park or a specific region filled in blanks and sharpened generalizations. Now Richard White has written the environmental history of a single county.

Island County in the state of Washington has several advantages as a subject for White. For one thing it was conveniently located near the University of Washington where White wrote the pre-book version of the study as a dissertation under Vernon Carstenson's direction. In addition, Island County is an island (more exactly, two islands) in Puget Sound. As such it offered a neat, insulated environment for a case study. Whidbey and Camano Islands are also representative of the American land in the sense no striking change or environmental disaster occurred here. The pace of change was gradual, life usually quiet. White has thus written about the typical rather than the exceptional, a fact that gives his book and methods broader applicability.

At the outset White takes up a lance in the chronic debate over determinism. He declares his allegiance to the theory that man and culture shape the land not vice versa. Next only to the glaciers, human beings molded Island County. Its environment is now synthetic, made by man.

White begins with an investigation of Indian use of the islands, and here he punctures his first myth. The Salish tribe did not appear to be particularly noble with regard to nature. They altered it freely when it served their economic interests to do so. When the Salish got guns from the whites they pushed the resident elk population to the brink of extermination. Yet in general White finds that

Indian use did not have as heavy an impact on the environment as did the pioneers. The lumbering, farming, and commercial fishing of white settlers produced permanent, far-reaching ecological change.

A second myth White corrects (at least for Island County) is that the pioneers ignored good advice and warning signs in their greedy exploitation of the environment. From this standpoint the pioneers *wanted* to change and destroy the ecosystem. As White sees it, however, the pioneers neither predicted, understood, nor desired the long-term impacts of their economy. They were surprised and shocked when their labors brought the productive capability of forests and soils below subsistence levels. The first to bemoan the exploitation of Island County were not conservationists but exploiters.

A final myth White challenges is that of the non-economic, "higher" motives of Island County's latest and present wave of occupants: recreation seekers. Scenic vistas and lady slippers, White argues, have real monetary value and are capable of being destroyed by overexploitation just as were fir, fish, and farms.

The challenge of writing readable, nationally significant local history is considerable. The local historian cannot pick and choose dramatic events and exciting personalities for the framework of his study. His raw material is the life and thought of very ordinary people and the often tedious record they leave in census data and production statistics. White succeeds in breathing much life into this data. He relates what went on day to day and year to year in Island County to regional and national developments. The book creates a fine model for environmental and social historians who choose to think small.

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AUGUST HECKSCHER. *St. Paul's: The Life of a New England School*. New York: Charles Scribner's Sons. 1980. Pp. xv, 398. \$20.00.

This is a first-class history of one of the country's great church schools. Most school histories tend to be uncritical celebrations of the institutions concerned; this one is different. The material has been thoroughly researched, the text is well footnoted, and there is a full bibliography and an index of names. The book is profusely illustrated with many striking photographs. The one of John Jay Chapman (p. 66), for example, is a classic portrait of a young American aristocrat. The whole volume is written *con amore*, as might be expected from one who was both a graduate and a trustee of St. Paul's

(SPS), and though readers unfamiliar with private schools may accuse August Heckscher of being sentimental in places, "old boys" will appreciate such sentiment. Despite the author's obvious devotion to SPS, he has succeeded in maintaining an objective stance throughout and has told the story of the school "warts and all."

Strong private schools usually owe their success to strong men who head them, and SPS was no exception. Heckscher quite properly concentrates on the work of three rectors: the saintly Henry Coit (1856-94), the austere Samuel Drury (1911-38), and the dynamic Matthew Warren (1954-69). Coit, who started the school with a handful of boys, and Drury, who dominated it during his entire term, shared the conviction that the Christian faith should be the cornerstone of the institution and the inculcation of that faith in the undergraduates its primary purpose. Though the school grew steadily in both size and plant under both, such concerns were secondary. Under Matthew Warren SPS underwent a dramatic series of reforms. Minority students were admitted, an ambitious building program was undertaken, and the rigidity of the religious program was eased. Under Warren's successor, William Oates, came the capstone of the reform period—coeducation. Certainly the SPS of today bears little resemblance to the ingrown community of Coit and Drury.

This reviewer cannot fault the author on what he has written. What he has omitted is another matter. There is almost nothing about the financial history of the school. Occasionally there are tantalizing references to this area, as when it is stated that Coit received no salary for nineteen years (p. 76) or that he would "pocket the receipts as they came in and disburse the necessary funds" (p. 143). SPS has always had one of the largest endowments per student of any school in the country, yet there is no account of how these funds were acquired.

At least until after World War II SPS was known as a school that catered to the sons of the socially and financially elite. But there is no attempt to explain how the school acquired this reputation or the extent to which it was justified.

Although one may regret omissions like the two cited above, such regrets are more than counterbalanced by the excellence of the story told. One hopes that the example of this book may encourage additional scholarly histories of a relatively neglected area of American education—the private school.

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H. C. PORTER. *The Inconstant Savage: England and the North American Indian, 1500-1660*. London: Duck-

worth; distributed by Southwest Book Services, Dallas, Tex. 1979. Pp. xix, 588.

In the preface to *The Inconstant Savage*, H. C. Porter of Cambridge University's history faculty asserts that he has "no pretensions to 'Ethnohistory'"; instead, his "intention has been to present descriptions of the Indians and of Virginia by Tudor and Stuart observers, and to illustrate English thinking about Indians." Porter's sources are major literary works such as More's *Utopia*, Shakespeare's *The Tempest*, and Defoe's *Robinson Crusoe* as well as continental and English chronicles of exploration. Such sources, combined with his stated intentions and the book's hefty dimensions, suggest a parallel with the early chapters of Winthrop Jordan's *White over Black* (1968). Porter seems to be examining English and colonial attitudes toward Indians much as Jordan explored English and American views of Africans. There the analogy ends. Whereas Jordan's book is subtle, graceful, and analytical, Porter's is mechanical, tedious, and themeless. *The Inconstant Savage* is a 548-page lesson in how not to write a book.

The pity is that the subject is vital to our understanding of Anglo-Indian contact from its beginnings to the present day, and H. C. Porter is obviously an intelligent, widely read, and sometimes witty scholar. He has carefully culled most of the important printed sources that refer, even obliquely, to the American Indian between the early sixteenth century and the 1660s. But in the process of telling about those sources he largely eschews analysis and instead leads us line by line through each text—usually one text per chapter—until we arrive, bored and exhausted, at the end of both. His next chapter is almost certain to open with several discursive pages of general or biographical background followed by the dissection of another early English book. This is not history; it is recitation, and often of the cut-and-paste variety in which block quotes are strung together with occasional transitional sentences (for example, pp. 28, 334, 472, 546). Worse, Porter has little sense of relevance. His book is cluttered with trivia, much of it concerning his own institution. He seems compelled to mention every affiliation, however remote, between his chroniclers and Cambridge; he even advises us that "the authority of a Royal Governor [in early Virginia] was not unlike that of the Head of an Oxford or Cambridge College" (p. 514). In the early chapters this Oxbridge fixation is quaint; after five hundred pages it is annoying. So are his liberties with English grammar. His oddities in punctuation and paragraphing would scandalize a freshman composition course. One example: an entire paragraph (p. 514) reads "At which point the aged Opechancanough decided to take revenge."

Porter's favorite stylistic device is the parenthetical aside, which clutters almost every page and is exceeded in nuisance value only by the extraneous publication information he crams into his text rather than his footnotes. A case in point is a paragraph (pp. 57–58) that gives, among other minutiae, the dates and publication places of several editions and printings of More's *Utopia* as well as the full citation of a colleague's review of a recent edition.

*The Inconstant Savage* is less a book than it is notes for a book. Those notes should have been winnowed, shaped, digested, drafted, revised, and edited before they were bound and sold. Neither Porter nor Duckworth Press fulfilled their responsibilities. And because they did not, *The Inconstant Savage* is likely to gather dust and its author's considerable learning to be largely wasted. Readers who want information about England's literary and psychological reaction to the American Indian will find a more concise and thoughtful analysis in Bernard Sheehan's *Savagism and Civility: Indians and Englishmen in Colonial Virginia* (1980)

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ROBERT C. RITCHIE. *The Duke's Province: A Study of New York Politics and Society, 1664–1691*. Chapel Hill: University of North Carolina Press. 1977. Pp. xii, 306. \$16.00.

The revival of interest in the history of colonial New York has generated some engaging new accounts of New Netherland and some provocative studies of eighteenth-century English New York. The years from 1664 to 1691, when the colony was a proprietary of the Duke of York and briefly a royal province, have remained in the shadow of historiographical neglect. In this meticulously detailed treatment of the duke's province, Robert C. Ritchie has given us about as comprehensive a history of these years as the surviving records allow. Largely political in its perspective, Ritchie's work—which won him the New York State Historical Association's Manuscript Prize for 1975—describes the transition from Dutch to English rule, the conquerors' attempts to create viable political institutions, and the instability that culminated in Leisler's Rebellion. Ritchie regards the quarter-century following the English conquest as a period of transition from settlement to society, during which the institutions of the Old World were refashioned to meet New World conditions. The generalization is acceptable enough, but the scope of the book does not really permit adequate attention to such institutions as the church,

the school, the slave system, the rural economy, or the family.

What Ritchie does demonstrate effectively is that neither the duke nor his advisers managed to develop the legitimate basis for an enduring polity. This was an era of "garrison government," as Stephen Webb has shown quite conclusively in his recent study, *The Governors-General* (1979); and New York's authoritarian regime, administered by a succession of chief executives all drawn from the ranks of the regular military establishment, validates the observation perfectly. The duke's governors sought to promote their own financial interests; their heavy-handed administrations represented the exercise of "power without principle" (p. 235). They were unable to comprehend the province's ethnic diversity or to reconcile the cultural tensions between Dutch and English. They could not satisfy the competing demands of the Long Island towns, the New York mercantile community, and the up-country fur traders. They could not even end the colony's trade with Holland and incorporate New York into the English mercantile system. The assembly, reluctantly convened in 1683, lasted less than two years, and the legislation it enacted lapsed with its demise. Leisler's Rebellion and the chaotic events that surrounded it are viewed by Ritchie as the baleful consequence of New York's political instability: a representative assembly might have provided New Yorkers with the vehicle for confronting political problems and diffusing political discontent. As it was, in the crisis of 1689, "the people had no common institutions to rely on, and the result was fragmentation of the province" (p. 200). The establishment of the bases of New York's political legitimacy would have to await several more decades of executive experimentation.

Ritchie's skillful reconstruction of the years of English New York's infancy goes far toward explaining the peculiar character of the colony's later political history. Handicapped by a late start, aware of their extraordinarily volatile society, and cognizant of their turbulent first quarter-century, New Yorkers worked all the harder during the eighteenth century to create a political system that would accommodate the colony's vastly diverse ethnic, religious, sectional, and economic interests. They succeeded remarkably, becoming thereby, in Michael Kammen's words, "a paradigm of what America at large would become in the nineteenth and twentieth centuries: pluralistic, conflict prone, materialistic, and individualistic within certain ill-defined bounds of conformity" (*Colonial New York: A History* [1975], p. xvi).

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TERRENCE ERDT. *Jonathan Edwards: Art and the Sense of the Heart*. Amherst: University of Massachusetts Press. 1980. Pp. xiv, 123. \$13.50.

Size is no measure of the potential contribution of Terrence Erdt's volume to the growing body of literature on Jonathan Edwards. Erdt is concerned with the relationship between the artistic imagination and the conversion experience within Edwards's thought. His study is an excellent example of interdisciplinary scholarship.

The volume opens on a retrospective note as Erdt discusses the Calvinistic preoccupation with the "sense of the heart," a concept Edwards inherited from Reformed theologians on the Continent and their Puritan successors. According to Erdt, the Calvinistic lexicon available to Edwards provided the beginning point for his theological reflections on the psychology of conversion. The accent upon Edwards's debt to the Reformed tradition places this volume in fundamental tension with the interpretation of Perry Miller, who argued that Edwards derived his notion of the sense of the heart rather exclusively from the empiricism of John Locke. In contrast to Miller, Erdt demonstrates that Edwards's principal contribution to the tradition of spirituality was his use of Lockean categories to formulate an understanding of what John Calvin called the *sensus suavitatis*, the perception of the sweetness of God given to the saints. Faith becomes the apprehension of that beauty. According to Edwards, the saint "consents" to God and after regeneration is able to love holiness. Grace makes such consent possible.

Thus the stage is set for a discussion of Edwards's aesthetic of conversion. For him the sense of the heart is a foretaste of heavenly happiness. Sin, however, makes any continuing delight on earth impossible for the saints. But the mind can be raised intermittently to a limited perception of divine beauty through substitute images evoked by the imagination or by works of art. Natural beauty, for example, can produce ideas of spiritual beauty. Art can also assist in another way by allowing the mind to imagine that which it has not experienced, the fires of hell being a case in point. Thus the preacher is able to use art for the ends of religion. According to Erdt, Edwards carried the art of evangelical preaching to new heights by removing all abstractions from the imaginations of his hearers and by bringing new intensity to the sermon form. He succeeded in making damnation imaginable, even though Erdt stops short of ascribing conscious artistry to Edwards's sermons.

The case for the connection between Edwards's aesthetic and his view of conversion is persuasive. Edwards's concept of the sense of the heart has substantial provision for the religious function of art.



Erdt appropriately suggests that this aesthetic feature of Edwards's thought might be a fruitful line for exploring the relationship between Edwards and other major figures in American literature.

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EDWIN J. PERKINS. *The Economy of Colonial America*. New York: Columbia University Press. 1980. Pp. xii, 177. Cloth \$17.50, paper \$6.00.

Potentially, at least, there is something to be gained from the nonspecialist surveying and summarizing the current version of our understanding of some subject. Linkages, developing patterns, and trends that can elude "the authority" occasionally have a way of coming clear to one who knows a bit less about a time or place. Nevertheless there are problems in such a venture. Its success depends upon the quality of the materials available for synthesis, and it is precisely at this juncture that the nonspecialist suffers from a distinct disadvantage. By definition, in crossing over casually into new territory, the historian leaves behind some of the heightened instincts and developed powers of discernment acquired through years of research and study.

Prior to doing this essay on the colonial economy, Edward J. Perkins wrote "a narrow monograph on the financing of nineteenth-century Anglo-American trade" (p. ix). He is too modest, of course, for his fine book on the Brown Brothers and a series of nicely done articles have staked for him a claim to becoming a major scholar of international finance in his century. But they have ill-equipped him for venturing into the mire that is early American economic history.

The contrast between the colonial period and the middle period in available data alone is enough to have kept most economic historians of the nineteenth century from wandering across the barrier marked "1789." Nationally collected trade figures do not compare with colonial compilations from naval officer shipping lists, the variations in which from port to port and officer to officer were only even beginning to be addressed in the late 1760s. Nationally collected census figures are able to be treated considerably differently from estimates based on militia counts, poll tax records, and parish registers. And these are only the most obvious and the best known problems. Yet profound difficulties in dealing with just those types of data—trade and population figures—infect two of the major recent studies of the colonial economy that Perkins draws on heavily for his essay. He appears not to know that neither commands much respect from any historian who has worked with the same sources.

We can forgive him that. We can even identify with his felt need to try a summation of the received wisdom on the colonial economy, and we can certainly applaud the attempt to accomplish it. Yet we have to recognize that one cannot build much of a structure if one's bricks lack straw. Perkins's failure stems simply from his being deceived about the quality of his building materials.

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DANIEL BLAKE SMITH. *Inside the Great House: Planter Family Life in Eighteenth-Century Chesapeake Society*. Ithaca, N.Y.: Cornell University Press. 1980. Pp. 305. \$17.50.

*Inside the Great House* is an ambitious book. Drawing upon a close reading of letters and diaries, Daniel Blake Smith reconstructs the character of family life in eighteenth-century Virginia and Maryland. The author is less concerned with historical demography than with contemporary perceptions, the meanings that members of great planter families attached to kinship and friendship. Smith is in search of "the personal values, beliefs, and emotions given expression in the daily life of the family" (p. 18).

According to Smith, the Chesapeake family went through three distinct stages of development. The first occurred during the seventeenth century when high mortality compressed the normal family life cycle and prevented husbands and wives, parents and children from forging strong emotional ties. But, as Smith argues, conditions improved substantially in the early eighteenth century. Planters lived longer; they became conscious of their standing within a large network of friends and relatives. Smith describes these families as authoritarian and patriarchal. Within them, order rather than love was the dominant value. Men and women achieved social identity only within a rigidly defined plantation hierarchy. The "Great Houses" were busy, public centers of activity, and friends, relatives, children, even slaves received equal attention from the great planter. William Byrd II apparently did not exaggerate when he declared, "I have a large Family of my own and my Doors are open to Every Body."

At mid-century the colonial Chesapeake family entered a third stage of development. Although Smith never explains exactly what caused this shift, he does believe that after 1750 planters defined "family experience" in strikingly new ways. The planter family became increasingly child-centered. Men and women openly expressed affection for other members of their immediate families. They regarded children as individuals requiring love and

nurture. As the Chesapeake family turned its attention inward, the planters and their wives lost meaningful contact with a broad network of kinfolk, and loving fathers replaced the fearsome patriarchs of Byrd's time. Young people talked of romance. They selected marriage partners without parental interference. Indeed, by the end of the century, Smith's planter family looks remarkably like Lawrence Stone's "Closed Domesticated Nuclear Family." After more than 150 years, the Chesapeake colonists had apparently rejoined the European mainstream.

Smith admits that his sources are less full than he would have liked. Colonial Virginians seldom were reflective people—at least on paper. The few who recorded their thoughts were elite planters, and the observations of a Byrd or a Carter may not reveal much about the character of less affluent Chesapeake families. Moreover, surviving letters and diaries reveal nothing about the effect of slavery upon the white family. Until historians know more about the racial dimensions of family life, they will be unable to make intelligent comparisons between Chesapeake and New England families. Smith provides an imaginative starting point for such an investigation.

T. H. BREEN  
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PAULINE MAIER. *The Old Revolutionaries: Political Lives in the Age of Samuel Adams*. New York: Alfred A. Knopf. 1980. Pp. xxii, 309. \$15.00.

Pauline Maier's book comprises a series of biographical essays about the public life of six revolutionaries. Also, in the middle of the work is a somewhat disruptive "Interlude" wherein the author edits the letters of Joseph and Mary Bartlett, one supposes, to describe the "age of Samuel Adams." The book focuses mainly on Samuel Adams, Isaac Sears, Doctor Thomas Young, Richard Henry Lee, and Charles Carroll of Carrollton.

The essay on Adams sets the tone for the book. One does not get too far before he learns that Adams is the archetype of the "old revolutionaries," no matter what the others' location, age, occupation, or economic views. In addition, the Adams essay turns on historiography describing how contemporary and later ages have regarded his revolutionary deeds. Following this come biographies of Isaac Sears, the businessman of this diverse group, and of Thomas Young, the intellectual traveling revolutionary. These are quite good, especially that on Sears because it contains stirring descriptions of the rioting in New York City where natural rights become bread and butter issues. Generally enjoyably written, there are some amusing lapses. In the great street fight in New York between the

mechanics and British soldiers, who had taken their jobs, Maier has the Golden Hind Tavern and its patrons brawling against the soldiers.

As the essayist moves southward the book becomes less effective. One must do much turning and crimping to fit the author's model. That Richard Henry Lee was "a misplaced New Englander" stands on its own (p. 165). All of the figures including the men of the South believed in representative government, the rights of property holders, and in general natural rights. Cicero, Bellarmine, Locke, Puffendorf were read everywhere, and grievances against England were asserted very early in the struggle as these ideas fitted local America. But the New Englander was not the chief teacher of the philosophy, however much Samuel Adams was admired by others.

The essay on Carroll is fun to read. How is he an "old revolutionary"? She theorizes that it centers mostly in his psychological make-up, his long education (never at Bohemia) by the Jesuits at St. Omers and English lawyers at the Middle Temple. His long years as an illegitimate son is a factor in his formation. "The obedient son" convinced his father to remain in Maryland at a time when the elder had reached the heights of despair over his and other Catholics' mistreatment by the popular lower house. Was it an obedient son or one of daring that convinced the papa not to leave and eventually join the popular cause? The essay does very little with Carroll's role as a revolutionary, the framing of the Maryland constitution, the authorship of the state bill of rights, the attempt at gradual abolition: feats that might the better make him an "old revolutionary."

Whatever the arguments the reviewer may have with the thesis, Maier has written a provocative book containing great hermeneutical value.

RICHARD WALSH  
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LINDA K. KERBER. *Women of the Republic: Intellect and Ideology in Revolutionary America*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1980. Pp. xiv, 304. Cloth \$19.50, paper \$9.00.

"The Spirit that prevails among Men of all degrees, all ages and sex'es," Abigail Adams wrote in 1775, "is the Spirit of Liberty." Perhaps. But as Linda K. Kerber demonstrates in this thoroughly researched and elegantly constructed volume, both before and after the Revolution, gender distinctions were made in defining citizenship that kept "Men" of the female sex in a subordinate position. In the colonial era, most women and a substantial minority of men were barely literate and politically apathetic. By the

end of the eighteenth century, however, war, technological developments, and constitutional experimentation had changed Americans. In particular, the eight years of largely guerrilla warfare, forced women to confront political issues. They shouldered burdens of citizenship. The rights of citizens, however, were another matter.

The egalitarian implications of American Revolutionary ideology were not drawn for females by those "Men" who held power. The doctrine of coverture, by which a married woman lost her legal existence, continued into the early republic; and divorce, by which a woman might free herself from a tyrannical husband, was still difficult to obtain. Furthermore, despite a narrowing literacy gap between men and women, learned women were ridiculed, and girls were warned that too much study would turn them into "masculine" creatures, repulsive to men.

Nevertheless, once American women were politicized and able to read, they were not content to confine their attention to mixing puddings and mending stockings. In their postwar writings, American women invented a new role model for themselves that justified taking an interest in matters outside the home. This ideal woman was intelligent and self-reliant; she had political opinions, a sense of patriotism, and a serious education. But most important, she did *not* threaten the family: the ideal republican woman was a republican mother whose political virtues existed entirely for the benefit of her children. More particularly, they existed for the benefit of her sons, whom she was rearing to take their places as full citizens in a free nation. The republican mother herself was a second class citizen. It was not full liberty; but for women who loved their country and cared about public issues, it was certainly preferable to total exclusion from politics.

LINDA GRANT DE PAUW  
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GERARD H. CLARFIELD. *Timothy Pickering and the American Republic*. Pittsburgh: University of Pittsburgh Press. 1980. Pp. viii, 320. \$19.95.

In 1801, upon returning to live in his native Salem, Massachusetts, after nearly thirty years away, Timothy Pickering ran for Congress. His Republican opponents, aware of his bitter, notorious quarrels with former Presidents Washington and Adams, observed that now the Federalists "want to send him to quarrel with Mr. Jefferson" (p. 217). And quarrel he did, intemperately, slanderously, and tediously, earning for himself the dubious distinction of having reviled more openly than any other public official each of the first three presidents: to him Washington was distinguished for his mediocrity, Adams

for his vanity, and Jefferson for his hypocrisy. For this quarrelsomeness, and for administrative incompetence, egomania, small-mindedness, and other flaws, Gerard H. Clarfield makes no apologies in this engaging, carefully researched biography of an exasperating yet finally remarkable and even appealing man.

For one thing, Pickering proved what an incredible amount a strong, healthy, determined, intelligent, human being can accomplish in fourscore or more years. He was in the Revolutionary army (largely as a "desk officer") for seven years, took a leading part in Wyoming Valley, Pennsylvania, land speculation and settlement, served as postmaster general, secretary of war, and secretary of state under Washington and Adams, negotiated important Indian treaties in western New York and the Ohio country, represented Massachusetts in the Senate or House of Representatives for twelve years, and lived long enough to take an active part in Andrew Jackson's 1828 campaign for the presidency. This service might have earned him a high rank among the founders had not his unbalanced, self-righteous paranoia also propelled him to intrigue against Washington during the Revolution, disloyally undercut President John Adams's peace overtures to France in 1799-1800, advocate New England secession in 1804 (by collaborating with Aaron Burr), and approach treason in opposing the War of 1812.

Yet Pickering could not be accused of dishonesty or corruption by even his bitterest enemies, and among family and close associates he exhibited warmth, care, and good will. He was, in fact, to a high degree "inner-directed," intent on living according to his own convictions. He was thus in one sense a classic high Federalist, entirely paternalistic, inattentive to popular opinion, and by the end of his life the very embodiment of a bygone age. But as a sincere, effective friend of the Indians, a determined foe of slavery, an outspoken Unitarian, and finally, an earnest Jacksonian, he was anything but conventionally conservative. Pickering, that is, though in some measure deserving John Adams's scorn as one with "a strong desire for celebrity" but only a "feeble means of obtaining it" (p. 266), was also true to himself and to his country according to his own often jaundiced view of what that required of him.

I wondered, in beginning this biography, whether it would be worthwhile to read another book by Clarfield about Pickering. It was, because Clarfield has used his unique mastery of the uniquely cumbersome and hard-to-read Pickering papers of the Massachusetts Historical Society to fashion a lean, incisive, interesting account of a life, given its length, dimensions, and contentiousness, almost certain to fall into tedium. Most notable is the author's

candor in laying out Pickering's considerable shortcomings, yet for all of that (and in a way *because* of it) giving us a believable picture of a man we can fairly count among the worthy patriots of the Revolutionary era.

RALPH KETCHAM  
Syracuse University

RICHARD BUEL, JR. *Dear Liberty: Connecticut's Mobilization for the Revolutionary War*. Middletown, Conn.: Wesleyan University Press. 1980. Pp. xiii, 425. \$22.50.

On the surface, *Dear Liberty* explains how Connecticut defended its liberty during the seven-year war for independence. Drawing primarily on the public records of the state and Congress, newspapers, and private correspondence, Richard Buel, Jr., traces the struggle to recruit soldiers, supply the troops, finance the war effort, and maintain morale. All of these efforts nearly failed, especially during the dark months of 1779 and 1780. Buel's report is not flattering. Peace solved the military problem but left the state bankrupt and the people fearful; more than a decade passed before the state began to recover. The Revolution inflicted political, economic, and psychological wounds that "would not heal during the lifetime of those who had passed through it" (p. 334).

On a deeper level, Buel is less interested in detailing the immediate economic and social effects of the war years than in their political consequences and the ideological changes that occurred, especially in the minds of the leaders. He argues that the weakness of government during the war, its inability to obtain essential men, money, and supplies, convinced these leaders of the need for a much stronger central government. They therefore became enthusiastic advocates of the federal Constitution and Hamilton's program that followed. The book reflects accurately and capably the Federalist analysis of wartime issues and the "critical" period and their motives in establishing the new system. Buel clearly agrees with them, and his book therefore ends as an explanation and celebration of their success, a timely forerunner of the coming bicentennial.

This being the case, one perhaps ought not to complain that the account of Connecticut's war effort, though far better than anything in print and an excellent study of its type, remains incomplete. We read much about losses, destruction, and impoverishment, but we find no hard facts about the people's economic circumstances—precisely who lost or gained, and how much. The principal source for such a quantitative analysis consists of several thousand estate inventories. They present problems but supply the necessary details. Also, we learn little about the people in general—the soldiers, sailors,

farmers, or artisans—for Buel is concerned primarily with the actions and ideas of the leaders. This is fundamentally a political, not an economic or a social history.

Finally, Buel's account of Connecticut's reaction to the postwar depression and threat of chaos, the turn toward authority, seems to gain force from historical and current parallels. But why was it, then, that other states followed a different course? Did Connecticut suffer more, face greater anarchy, and so react more strongly, than its neighbors? We do not learn anything about all of that, whether its experience was unique, typical, more or less successful. And if the wartime experience accounted for Connecticut's federalism, why did other states, including its neighbors, New York and Rhode Island, oppose the Constitution? Clearly some other factors were at work that the account of a particular state cannot explain. We end unconvinced of Buel's interpretation, while still admiring his interesting and valuable account of the state's mobilization and his excellent prose style.

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WILLIAM STINCHCOMBE. *The XYZ Affair*. (Contributions in American History, number 89.) Westport, Conn.: Greenwood Press. 1980. Pp. 167. \$23.95.

In recent years professional historians have usually focused on narrow topics or on painstaking revisions of subjects that earlier scholars have well investigated. The results generally appear in a scholarly journal and occasionally as a brief monograph. The latter is the case with this new study of one of the best-known episodes in American history, the special mission of Charles C. Pinckney, Elbridge Gerry, and John Marshall to France in 1797 that became notorious as the XYZ affair.

Essentially, William Stinchcombe retells an old tale and, in the main, reaffirms what other scholars have presented. He differs with his predecessors on small points such as in the correct use of names, in the explanation of an occasional document previously misunderstood or somehow overlooked, and in a richness of detail concerning minor figures. For example, Stinchcombe clears up some confusion concerning the true identity of "W," one of the unofficial agents of Charles Maurice Talleyrand, France's foreign minister, who approached the American commissioners in Paris. The author convincingly identifies "W" (p. 55) as Charles Hubbard, an Englishman working as a junior partner in a Dutch banking firm. Stinchcombe also offers a correction (p. 65) on the roles of Madame de Villette and Madame de la Forest, often made to seem mysterious in the informal diplomacy that had be-



come a feature of the American mission. More important, he clarifies the relationship of John Adams and Alexander Hamilton (p. 23) at the time, demonstrating as a number of other historians have not, that Adams knowingly accepted advice from Hamilton. At least in the matter of instructions to the envoys, Adams was not, as some have maintained, the dupe of a cabinet manipulated by Hamilton. More than any other study of the negotiations, Stinchcombe's volume analyzes the activities of the members of the American community in Paris during this period and explains their views on the French-American crisis. The author claims that the attitudes of those expatriate Americans fundamentally influenced the negotiations (p. 81), but in the view of this reviewer at least, he does not demonstrate with sufficient clarity how they did so. In effect, he appears to have analyzed quite well the nature of advocacy by peripheral figures but not as well how they were able to transform their views into policy.

Except for this brief and pleasant excursion into the lives of those who belonged to the "American club" in Paris, this is a study in traditional diplomatic history. It concentrates on negotiations and builds its analysis from a foundation of dispatches and other correspondence, most of which have been previously exploited by scholars. It does not concern itself with domestic influences on foreign policy, such as the cause of war hysteria in the United States or with the impact of political issues on the American stance in Paris.

This study is sound in scholarship, being founded on a careful re-examination of archival material, of printed sources, and of all the pertinent secondary literature. Moreover, it is a lucidly written account, skillfully structured, sophisticated in analysis, and easy to follow. Despite its self-imposed limitations, I for one like this small book and found it a pleasure to read. It will be useful to scholars as a convenient, reliable, graceful, and up-to-date synthesis of currently accepted scholarship about a single but important episode in the early days of the republic.

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EUGENE D. GENOVESE. *From Rebellion to Revolution: Afro-American Slave Revolts in the Making of the Modern World*. (Walter Lynwood Fleming Lectures in Southern History, Louisiana State University.) Baton Rouge: Louisiana State University Press. 1979. Pp. xxvi, 173. Cloth, \$9.95, paper \$3.95.

This bold and brilliant essay, reshaped from the Fleming lectures given at Louisiana State University in 1973, upholds Eugene D. Genovese's reputation as the premier generalist in the slavery field

and as a perennial gadfly. In less than 150 pages of text he attempts not only to provide a hemispheric perspective on the myriad Afro-American slave revolts hitherto studied piecemeal but also to assess their part in the making of the modern world. Few will be convinced by the book's basic Marxism or by the central thesis implied in the title, and many will be frustrated by the complete absence of reference notes. Some might even quip that *From Rebellion to Revolution* is a brief and allusive book because it could not be sustained at greater length and with greater detail. Yet, as ever, even where we disagree, Genovese makes us rethink our fixed ideas, think large, and simply relate.

In making the most comprehensive survey yet of active slave resistance in the Americas, Genovese in part echoes earlier analyses in listing causes and types, although he clearly becomes dissatisfied with their explanatory value. In the first of the book's three main parts he lists eight "general conditions that favored massive revolts and guerilla warfare." Yet, as he readily admits, none of these were *necessary* conditions, and slave revolt on a lesser scale (as in the United States) could stem from very different conditions. Nor, for that matter, was revolt the only possible manifestation of slave resistance—slaves intelligently chose passive resistance when revolt was tantamount to suicide. By subsuming his eight general conditions into the general principle of "the military and political balance of power," Genovese comes close to suggesting that slave revolt was an inevitable response wherever conditions were right. One wishes he had taken the logic several steps further: to suggest that slave resistance was coterminous with slavery, its forms being simply determined by existing conditions.

This might have allowed Genovese's tentative explorations into typology to have been more satisfying. In the book's second part he makes an overly clear distinction between slave revolts and maroon activity, and throughout he clearly distinguishes between African-inspired revolts and the more sophisticated responses made by Afro-American slaves. For Genovese, maroons differed from other rebels by being prepared to collaborate as the price of escape from plantation bondage, and creole slaves differed chiefly from Africans in being influenced by European ideas and modes. For reasons that will soon be obvious, he shies away from another interpretation: that all slaves were seeking essentially the same thing, the right to make a life of their own in the world in which they now belonged. The taming of the wild woods by the maroons, the process of acculturation called "creolization," and paradoxically even accommodation itself (or apparent accommodation) could all enlarge the ambit of freedom and thus can be regarded as indigenous forms of resistance.

Yet Genovese's primary purpose is not to examine causes and types (which too easily lead to static models) but rather to propose a dynamic historical pattern for slave revolts. His major thesis, developed most clearly in the book's third part, is that "by the end of the eighteenth century, the historical content of the slave revolts shifted decisively from attempts to secure freedom from slavery to attempts to overthrow slavery as a social system." The earlier revolts, he maintains, simply aimed to restore African ways and were therefore mere rebellions like medieval peasants' revolts. Only the later revolts were truly revolutionary in that the slaves played an important part in the "bourgeois democratic revolution," which pushed history forward and helped shape the modern world. The crucial turning point was the Haitian upheaval of 1791-1804.

This thesis—attempting to place and explain Afro-American slave revolts in a way that Marx neglected to do and that C. L. R. James and Herbert Aptheker did only in part—lands Genovese in serious difficulties with his evidence. Some of these are acknowledged in the dazzling preface tacked onto the book, others not. Most early slave rebels did aim to overthrow the socioeconomic system of chattel slavery and plantations, by destroying them totally. At the same time, the late slave revolts did much more to come to terms with the prevailing capitalist world system and even adopted much of its underlying ideology. In the case of Haiti, by Genovese's account, hoping to forge a black republic that imitated European forms and employed European slogans is somehow more "revolutionary" than an attempt to erase the socioeconomic system and create, or reinstate, an Afro-American alternative.

If there was, in fact, a turning point in the change from a predominantly African to an Afro-American rejection of the slavery system, it surely came much earlier than 1791 (or 1789, or 1776). For example, the planters of Barbados uncovered a plot in which the slaves (who already called themselves Barbadians and claimed that the island belonged to them) drew up a blueprint list of leading rebels who would take over the specific roles of all the white leaders, from the governor downward. This was in 1701. Besides, the system that the slaves in the later rebellions aimed to substitute, and that was largely achieved by emancipation in many areas, was an Afro-American peasant culture that owed at least as much to the environment as to Africa, Europe, or the capitalist world system in its intermediate phase.

Although greatly stimulated by *From Rebellion to Revolution* and particularly grateful for the twenty-five-page bibliographical essay with which the author almost recompenses for the absence of notes, one comes away with two unexpected conclusions. By largely ignoring the continuity of slave resis-

tance, dismissing the African rebels as merely "restorationist," playing down the effects of creolization and, in effect, denying black slaves an ideology of their own, Genovese, like Marx, is curiously Eurocentric. Even more iconoclastic is the thought that with such an obvious predilection for the "progressive" elements in history, to the exclusion of others with less "historical content" (albeit continuous and timeless), Genovese is as quaintly dated as Marx in his mid-Victorian optimism.

MICHAEL CRATON  
University of Waterloo

FRED J. HOOD. *Reformed America: The Middle and Southern States, 1783-1837*. University: University of Alabama Press. 1980. Pp. 254. \$21.50.

Drawing upon nearly three thousand printed sources, Fred J. Hood argues that Reformed Protestants south of New England sponsored and attempted to implement a distinctive vision of the place of religion in the new American republic. John Witherspoon and Samuel Stanhope Smith, successive presidents of the College of New Jersey (Princeton) from 1768 to 1812, wove Reformed doctrine together with themes from the Scottish Enlightenment and American democratic theory into a charter for an informal religious establishment. They envisioned republican institutions and national prosperity as dependent upon a broad consensus of theological and moral values conceived along Reformed lines. In the Reformed denominations, representing the most socially prestigious constituency in the region, the Princeton doctrine dominated; it was far more prominent in the early national consciousness than the ideas of Jefferson and Madison. Under its influence, church leaders pressed governmental officials formally to avow divine Providence, to support Protestant Christianity by deed and example, to shape legislation to conform to natural and divine law, and to enforce the Sabbath. Often frustrated on these fronts, they turned after 1800 to the control of public opinion through voluntary societies stressing religious indoctrination, but this effort was undermined by contention over issues arising from New School revivalism. By the time of the great Presbyterian schism of 1837, the "Reformed scheme to conquer America through indoctrination" had failed (p. 202).

Several of Hood's arguments will evoke controversy. The reliance of Reformed thought at large upon Witherspoon and Smith is not demonstrated fully. The notion of "middle and southern" Reformed America as a distinctive theological collectivity could bear further clarification. Not all of the ideas outlined are as differentiated from New England patterns as Hood suggests; little is said of the impact of British thought, save for the Scottish ele-

ment at Princeton. Most debatable is the author's rather unrelieved confidence in "social control" as an explanation for Protestant behavior. Thus viewed, voluntary societies become cynical devices for "indoctrinating" and "controlling the masses" (p. 169) with deliberately "veiled" (p. 165) propaganda. This was a part of the picture, no doubt. But we may question whether the aspirations, and especially the theological seriousness, of the churchmen studied can be represented fairly in such terms.

Yet at most points this is an important, carefully constructed, and well-considered work. It fills a sizeable gap in historical knowledge. Certainly the Reformed tradition in the southern and middle states was not a New England appendage, as is often assumed. The common notion that benevolent societies were products of revivalism here meets a delightful comeuppance: Hood finds "no connection between revivalism and the society movement before 1826" (p. 170). Similarly, the widespread assumptions that revivalism was primarily an individualizing force, or that it was opposed by the traditionalist Reformed, are found to be without foundation in evidence from Reformed America. Here for the first time the distinctive conservative version of the revival—an orderly "season of refreshing" limited to church members and their families and controlled by careful theological instruction—is recognized adequately. Students of early national America will find their horizons widened at several points by this erudite, clearly written book.

THEODORE DWIGHT BOZEMAN  
University of Iowa

ROBERT E. SHALHOPE. *John Taylor of Caroline: Pastoral Republican*. Columbia: University of South Carolina Press. 1980. Pp. ix, 304. \$19.50.

As Robert E. Shalhope points out in his introduction, the conflicting scholarly assessments of John Taylor range from being called a "parochial windbag," of "almost no philosophical influence upon his contemporaries," who "embodied an archaic economic view," to "the most brilliant mind in the Republican organization" and "a penetrating critic of Hamiltonian finance." Shalhope promises to bypass these contradictions and to take a "fresh approach" by applying the analytic tools developed by Thomas Kuhn, Gene Wise, Kenneth Burke, and others. The reader must wade through several introductory pages of heavy methodological water about "perspectivism" and "existentially rooted" views and biases. Fortunately the body of the work is traditional intellectual history at its finest, which simply means that Shalhope explains Taylor's political philosophy, its historic roots, the contending currents of thought, and the particular

circumstances influencing his position on issues. This reviewer especially appreciated the clearly drawn distinctions between Taylor's and Jefferson's republicanism and Shalhope's step-by-step analysis of Taylor's rebuttal to John Adams (*An Inquiry into the Principles and Policy of the Government of the United States*). Shalhope's book neither will resolve nor end historical disagreements on Taylor. It does not fulfill what is still needed, a full-length biographical treatment. But it is, and richly deserves to be, the definitive study of his philosophy, placed in a context that enhances our understanding of the political history of the early national period.

One cannot really separate Taylor's writings on agriculture or federalism or sovereignty or republicanism or political economy: all were part of a single structure that he developed and consistently defended throughout his life. Virtue and morality loom large in his thought, as they did for most of his contemporaries who were concerned that without these qualities republicanism could not survive. For Taylor, apparently, morality did not stem from Christianity—Shalhope says nothing of his religious beliefs or lack thereof—but from an agrarian lifestyle of purity and simplicity. Thus, the subtitle of the book, "Pastoral Republican." In a sense Taylor always remained a Southern Antifederalist: an agrarian localist and libertarian (for whites) who never accepted the compromises and balancing principles inherent in the United States Constitution.

Shalhope sees some shifts in Taylor's perceptions from the 1790s when he "remained within the mindset of an eighteenth-century oppositionist" to the period after the War of 1812 when "slowly he realized that industrialism and capitalism were at the base of the situation" (pp. 213–14). That may be, but his logic remained similar, his rhetoric the same, even the identical phrases he first used in opposition to Hamilton's financial program were repeated in later pamphlets. The enemies always were materialism, capitalism, the tendency to consolidation in a federal government controlled by financial interests. More important is the fact that southern attitudes did shift. In 1814 Jefferson thought Taylor's *Inquiry* "quaint" and "farfetched." By 1821 he recognized his indebtedness "for many invaluable ideas, and for the correction of some errors of early opinions, never seen in a correct light until presented to me in that work." Taylor mirrored the experience of many Southerners and articulated their fears. He "may well represent in embryo the cultural system that chose to secede," Shalhope concludes, "rather than to relinquish a way of life believed to be right and good" (p. 217).

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GARY LAWSON BROWNE. *Baltimore in the Nation, 1789-1861*. Chapel Hill: University of North Carolina Press. 1980. Pp. xiii, 349. \$20.00.

As Baltimore faces an uncertain future, it is experiencing a renaissance of historical interest in its past. Gary Lawson Browne presents a scholarly treatment of its transformation from a commercial to an industrial city. He divides his study into three periods—1789-1815, 1815-43, and 1843-61—that represent developmental stages in this process. Overall, he advocates employing an interdisciplinary approach, with special emphasis on the dual themes of modernization and the industrial revolution, to survey the transformation of Baltimore's social economy.

Baltimore became a dynamic commercial city, specializing in wheat and developing an extensive trade with the West Indies during the Revolution. Conforming to the stereotype of a "private city," one with weak institutions and modest services, a merchant elite ruled in its own interests. Various historical events, such as the War of 1812 or the Panic of 1819, or processes, such as the monetization of credit or the increasing popular demand for public services, undermined their role so that by the 1850s the middle class, epitomized by the rise and success of the Know-Nothing party, governed the city and its institutions. Baltimore's transformation from a commercial to an industrial economic base was nonetheless slow and halting. Begun in the early nineteenth century, it was inhibited not only by foreign competition but also by divisions within the merchant community that questioned its promise for success. In many ways Baltimore's fixation with its agricultural hinterland, first in contiguous areas and later by expansion via the railroad into the Ohio Valley, limited its vision for change. Despite the vicissitudes of trade, reliance on traditional commercial relationships generated sufficient return to inhibit real change, so that the threshold of industrialization was not crossed. Baltimore remained a commercial rather than an industrial city prior to the Civil War.

Social scientists may find this interpretation disappointing. Although Browne avows an interdisciplinary approach, with an interest in modernization, the social economy, or the industrial revolution, concepts such as these are not systematically elaborated or employed. For instance, it is unclear whether Baltimore's evolution as an industrial city was inhibited more by divisions within its merchant class than by changes in regional transportation that placed its workers in direct competition with producers in other areas. Similarly, unlike recent books by Susan Hirsch or Allan Dawley that analyze the process of industrial changes more directly, Browne never clearly explains suf-

ficient conditions for the transformation of Baltimore from a commercial to an industrial economy.

The strength of this book is its focus on how Baltimore's merchant class viewed and reacted to change between the Revolution and the Civil War. It is more properly a business history than a comprehensive study of modernization. Now that we know how this elite felt and can build on Browne's extensive research into primary sources, we are in a better position to understand the complex structural and social changes associated with industrialization and to begin to appreciate Baltimore's place in the nineteenth-century urban experience.

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KERMIT L. HALL. *The Politics of Justice: Lower Federal Judicial Selection and the Second Party System, 1829-61*. Lincoln: University of Nebraska Press. 1979. Pp. xvii, 268. \$19.50.

In a discipline too frequently characterized by overworked topics, it is a delight to read—and report on—a book that truly breaks new ground. *The Politics of Justice* is a prosopographic study of the two hundred judges appointed to lower federal courts from 1829 to 1861 and of the selection process itself. It is a valuable resource that will be especially useful to those interested in the second party system, the presidency, antebellum legal history, and all who are willing to scrap conventional wisdom in the face of new evidence.

Kermit L. Hall's excavations in the National Archives, manuscript census reports, more than three dozen newspapers, and collections in more than two dozen archives have brought to light information about the origins, social class, wealth, legal training, judicial experience, and political connections of the men who served in the federal district courts and in the territorial courts during the thirty-two years following Jackson's election. Through imaginative collations of data, the author reveals the individual selection styles of eight presidents during the era (Harrison made no appointments). In order to accomplish these analyses, Hall also surveys the political landscapes of all the states and territories where appointments were made and from which the presidents, their advisers, and the judges came.

A reader might drown in such a torrent of information but Hall is selective in narrating his findings. There is a chapter for each president, describing his principles of selection and illustrating those principles with carefully chosen examples. Exceptions are duly noted, results summarized, and the methodologies and statistical data are presented in appendixes. The back of the book also contains



footnotes, a bibliographic essay, and an extensive index.

Among the surprises is evidence that the practice of senatorial courtesy was not fully developed until after the Civil War, that there was no widespread vulgarization of bar or bench in Jacksonian America, and that—rhetoric to the contrary—Whigs were more likely than Democrats to appoint judges of modest origins. Yet even in that expanding era, most judges came from prominent or elite families; no one of truly humble background gained appointment to the federal courts. Not until the mid-forties did attorneys-general take over major responsibility for this part of the federal patronage from secretaries of state. But all presidents and their advisers recognized the political ramifications of jurisprudence and sought to identify men who shared their own specific ideological commitments.

This is a remarkable book to contain so much data in so few pages. Hall is truly talented in successfully integrating interdisciplinary insights. Such professionalism makes it unlikely that anyone will ever need to rework *this* subject again.

MARY K. BONSTEEL TACHAU  
*University of Louisville*

STEPHEN NISSENBAUM. *Sex, Diet, and Debility in Jacksonian America: Sylvester Graham and Health Reform*. (Contributions in Medical History, number 4.) Westport, Conn.: Greenwood Press. 1980. Pp. xvii, 198. \$22.95.

Sylvester Graham enjoys a secure, if small place in American history. An enthusiastic Jacksonian "health reformer," he is mentioned in every survey of antebellum social reform and is known even more widely in eponymous form as the original advocate of graham flour—and thus the omnipresent graham cracker. Stephen Nissenbaum has now written the first serious, full-length intellectual portrait of Graham, a figure whom he sees as peculiarly revelatory of American social experience in a period of traumatic change.

In several widely quoted books (and in successful public lectures), Graham elaborated a physiological rationale for denial and abstinence, one that promised health and long life through the avoidance of meat and rich, highly spiced foods and through the denial of sexual stimulation—by a regimen generally so chaste as to attract the ridicule of many of his antebellum contemporaries. This was, in Nissenbaum's view, an ideology of adaptation, of adaptation to an impersonal and labile world dominated increasingly by the capitalist marketplace. The emotional logic of Graham's position is powerfully expressed in his emphasis on the ordinary man's loaf of bread, once baked by a mother's hands from

home-grown wheat, but by the 1830s increasingly baked from commercially milled western flour by unseen hands. Thus a simple loaf of bread was both metaphor and reality, symbolizing and incorporating a particular kind of social change. To this point, Nissenbaum's interpretation recalls the work of a number of other historians—Peter Cominos on Victorian sexual ideology, for example—who have suggested a functional relationship between the form of putatively scientific Victorian ideas and the behavioral and psychological demands of the capitalist marketplace. But in Nissenbaum's view, Graham's logic points toward a rejection of the marketplace rather than a sanctioning of behavior appropriate to achievement within it.

Much of this study is an exegesis of Graham's ideas, with an emphasis on their sources in the vitalist physiology and pathology of M.-F.-X. Bichat and F. J. V. Broussais. Though Graham himself does not explicitly discuss the medical sources for his social physiology, Nissenbaum makes a strong textual case for his protagonist's having either read these authors or their popularizers in the setting of Philadelphia's lively and cosmopolitan medical community in the late 1820s and 1830s. In addition, Nissenbaum suggests a biographical parallel to the ideological content of this argument.

Born in 1794, Graham was the youngest of seventeen children of a New England clergyman—one who had filled the same West Suffield pulpit for almost a half-century. Sylvester was the last to arrive of his father's second family; the philoprogenitive clergyman had had ten children by his first wife and seven with his second. Nissenbaum emphasizes the contrast between the stability of Sylvester's father's long pastorate and the general prosperity enjoyed by his first brood of children and the comparative rootlessness of the senior Graham's second family. Nissenbaum suggests that this family history can be seen as a microcosm illuminating the macro-social changes that so alarmed many Jacksonian Americans. Graham's ideology of restraint and zealous self-denial thus represented a response to this disquieting perception of change. Nissenbaum has, in short, written a thoughtful book that succeeds in rescuing an intriguing figure from the partial oblivion of the historian's footnotes and whimsical asides and places him firmly in the center of analytical concern.

But this study is not without its problems. First, we know relatively little about Graham; biographical materials are almost nonexistent. Nissenbaum's bold biographical suggestions are based on the bare bones of minimal fact and a few retrospective anecdotes from Graham's own writings. Second, the problem of representativeness is never really addressed. For Graham's ideas were seen as extreme by contemporaries both lay and medical. It is hard

to specify a particular social location in which to lodge the purported psychic efficacy of these ideas. Like a number of other recent social historians, Nissenbaum employs the method of history of ideas to help shed light on underlying social realities; it is an attractive but elusive task. (Nissenbaum does provide a study of the social recruitment of the 124 members of the Boston-based American Physiological Society, a Graham-inspired self-improvement group, and shows them to have been largely skilled artisans and small proprietors, many of them newly arrived in Boston. But since the vast majority of Americans of this social class cannot be shown to have had any sympathy for Graham's notions, it is not clear that a meaningful social variable has indeed been isolated.) Third, Nissenbaum fails to evaluate the religious background out of which Graham's ideas and career developed. When Graham christens himself a "physiologist," Nissenbaum apparently takes him at his word and ignores the evangelical perfectionism that helped to produce Graham's hygienic crusade (not to mention the temperance and abolition impulses so attractive to many Grahamites). Finally, the medical ideas of Bichat and Broussais are granted too prominent and too specific a role in shaping and in some sense creating Graham's system. They seem, to this reader at least, far more a rationalization for pressing social conviction than a critically determinative element in Graham's thought.

CHARLES E. ROSENBERG  
University of Pennsylvania

JAMES R. MELLOW. *Nathaniel Hawthorne in His Times*. Boston: Houghton Mifflin. 1980. Pp. xiii, 684. \$19.95.

We can be grateful to James R. Mellow for showing us a Hawthorne who admired pretty girls, defended tipling by word and deed, and lent his purse and his sympathy to a doctor of divinity much the worse for wear after a week in a Liverpool brothel. But fashionable modern latitude intrudes excessively when relationships—both the author's and those of his fictional characters—are pointlessly assigned erotic dimensions. "I love him," Hawthorne writes in a discussion of Franklin Pierce, and yet we never learn why this lifelong association contains no hint of the clandestine while the distressingly undocumented friendship of Hawthorne and Herman Melville does.

It is easy to understand the appeal of this interpretation. Hawthorne had his mysteries and has them still. And then every literary biographer faces two perhaps insurmountable challenges. Authors are often dull people. They provide their chroniclers with few colorful events, no battles or significant

public adventure. There are only the books. Mellow is skillful in finding biographical implications in Hawthorne's tales, but the sad truth is that an artist is never celebrated for his success in capturing his life in his fiction. No biographer could know the basic sources more thoroughly, but Mellow has not devoted similar attention to the library of criticism that attests to both the fascination and the challenge of Hawthorne's artistry. Perhaps no single student could, but it is the peculiar burden of the literary biographer to justify his conclusions by interpreting art works often highly resistant to easy application. They are events of a most elusive kind.

Looking toward "his times" as a contributing source of interest for the life of a writer succeeds only if the subject has been notably alert to contemporary history or the biographer diligent in recreating it as a context for his subject. Hawthorne responded when pressed—by the need to secure political appointment, for example—but he seems to have been temperamentally private and apolitical. Mellow's attention warms most rewardingly when he brings us his author's contemporaries, the "fish and fowl and birds of angelic feather" who walked the streets of Hawthorne's Salem, Concord, and Boston. Here we have a fitting subtitle for the biography, one that points toward the composite portrait Mellow plans to complete with volumes on Emerson, Thoreau, and Margaret Fuller. Taken together, such a group of unavoidably partial portraits will show us this small but seminal corner of our cultural past more vividly than we are ever likely to glimpse its constituent personalities.

RICHARD RULAND  
Washington University

JOHN PHILLIP REID. *Law for the Elephant: Property and Social Behavior on the Overland Trail*. San Marino, Calif.: Huntington Library. 1980. Pp. x, 437. \$18.50.

American pioneers moved into the raw wilderness in their trek westward. Beyond the pale of settlement, the absence of law severely tested the persistence of culturally learned patterns of social behavior and increased the proclivity of many to become law unto themselves. Absence of law degenerated into anarchy, and violence thrived in such circumstances. Self-styled vigilante and other *ad hoc* groups arose to restore order pending the arrival of duly constituted authority.

John Phillip Reid says that the "supposed propensity" of Americans toward violence and the familiar inferences of lawlessness that follow from this assumption cannot be supported from the evidence. He asserts that historians and writers of fiction have either deliberately misled us about conflict resolution on the frontier, or they have misinterpreted or

been misled by the evidence. They have read violence into events that were not violent. They have not understood the legal behaviorism that determined conduct; they have assumed that confrontations not ending in violence meant retreat by one side, not mutual respect for notions governing legal behavior.

Reid focuses on one stage of the westering experience and concludes that much of the conventional wisdom in these matters may have to be modified considerably. On the trek across the Great Plains to Oregon and California, one of the great migrations of history, the physical baggage was battered, worn out, streamlined, abandoned, recycled, or replenished as circumstances dictated. The cultural baggage, however, remained remarkably intact and operative even under the most trying physical, emotional, and social conditions. Exceptions to this were rare.

In the vast legal no man's land between the Missouri and their destinations, these overland trekkers knew very well that they were beyond police surveillance and threat of prosecution and removed from the restraint of family, neighborhood, and church.

The persistence of the cultural conditioning of these pioneers—the consequence of religious principles, internal restraints, and their growing up in law-abiding communities back East—is inadequate explanation. Asserting that there was no law on the overland trail says no more than that there were no lawyers or courts for the resolution of disputes.

Reid is concerned, in particular, with property rights and social behavior. He finds abundant documentation in letters, journals, and memoirs of the westward migration that the emigrants understood definitions of property law. They distinguished between private ownership, partnership, company, and other concurrent methods of property holding. This knowledge guided their behavior in their dealings with other emigrants as well as in their conduct toward property. In short, Reid reaches two significant conclusions. Nineteenth-century Americans had an impressive legal knowledge. Even more noteworthy, the habits, actions, and values of nineteenth-century American society were formed by conduct based on law.

"To see the elephant" was a common expression most often used to describe the California gold rush and the overland emigrations. It meant facing a particularly severe ordeal, gaining experience through hardship, learning the realities of a situation firsthand, or encountering the unbelievable. Lawyer-historian Reid's analysis is an important revisionist study for American legal, social, and frontier history.

DWIGHT L. SMITH  
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Oxford, Ohio

PAUL K. CONKIN. *Prophets of Prosperity: America's First Political Economists*. Bloomington: Indiana University Press. 1980. Pp. xii, 333. \$25.00.

In *Prophets of Prosperity: America's First Political Economists*, Paul K. Conkin, a historian of ideas, attempts to apply the technique that worked so well in *Puritans and Pragmatists: Eight American Thinkers*. Unfortunately, the method of *explication de texte* is much less appropriate in dealing with the development of the science of economics than with some other facets of intellectual history.

Conkin provides a lay reading of the work of twenty American economists active between 1815 and 1860, including Henry C. Carey, Daniel Raymond, and George Tucker, as well as the relatively obscure. As Conkin notes in his preface, "In no sense have I explored new territory" (p. xii), and yet he sees this book as "a rescue operation" because early American economists have been ignored. Conkin acknowledges his predecessors, most notably Joseph Dorfman, who devoted the major part of his career to the study of American economic thought, but leaves the pervasive influence of that pioneering giant undocumented. Relying heavily on the writings of those political economists he studies, since he prefers to view them directly rather than refracted through the eyes of others, Conkin seemingly downplays the analyses of previous scholars if his footnotes are any guide, particularly those sources by and for economists.

This series of intellectual portraits would be improved if Conkin employed a narrower, sharper, and more consistent economic focus. In comparison with the treatment of earlier scholars, he includes much biographical material and much that is only marginally economic according to modern concepts of the nature and scope of economics. Conkin neglects to place his subjects in context, that is, he fails to relate them adequately to their predecessors and especially to their successors. Greater analysis of the intellectual validity of the argument and indication of the current standing of ideas rather than summarization would strengthen this work. This book contains no bibliography, which is perhaps no more than inconvenient for the reader.

Conkin's reading of these economists would profit by his knowing still more economic theory and more economic history. If, in fact, Conkin has used the work of other scholars for the purpose of his analysis, it is difficult to tell what part of his comments are his own as against those derived from the specialized research of others because he does not cite the scholarly literature except for biographical information. Finally, Conkin is at his best with such men as Thomas Skidmore and George Henry Evans, fundamentally agrarian reformers rather than political economists.

Conkin concludes: "Thus our first economists, of-

ten because of their disagreements, their wide-ranging theoretical differences, offer present Americans a peculiarly revealing, often poignant, insight into themselves" (p. 314). Should the reader seek this perspective, he will be well served because Conkin offers a perceptive and well-written commentary. If, however, the reader has other objectives in mind, he will be well advised to turn to *The Economic Mind in American Civilization*, Dorfman's older but by no means outmoded work.

SAUL ENGELBOURG  
Boston University

FREDERICK P. SCHMITT *et al.* *Thomas Welcome Roys: America's Pioneer of Modern Whaling*. (Mariners Museum Publication, number 38.) Charlottesville: University Press of Virginia, for the Mariners Museum, Newport News, Va. 1980. Pp. xiv, 253. \$15.00.

Thomas Welcome Roys, one of the leading—though largely forgotten—characters of American whaling in the nineteenth century, has been well served in this, the most interesting and informative whaling biography to appear for many years. It goes beyond the usual "whaling exploits" to reveal a restless man of inquiring mind and enterprising spirit. Between 1841 and 1854 he was a successful captain in Pacific whaling, leading the way into the Bering Strait. But recognizing the diminution of traditional whale stocks, he devoted the remainder of his life to investigating methods of catching the plentiful but elusive rorqual whales. The authors present a detailed account of his voyages and of his patient experiments with explosive rocket harpoons fired from traditional whaleboats; with a mechanism for raising dead rorquals that probably inspired Svend Foyn's "accumulator"; with steam whalers; with a land station (in Iceland, 1865); with shipboard digesters; with whale guano; and, fleetingly, with a steam catcher. All but the rockets were taken up by modern whalers.

Nevertheless, despite his experiments Roys remained one of those unfortunate visionaries who inspire others (notably Hammer, Bottemanne, and Foyn) but who cannot themselves make the jump from an old to a new technology. "It is difficult," write the authors, "to compare Roys and Foyn—the two great pioneers of mechanised whaling" (p. 141). Indeed it is, and this reviewer is not convinced that the evidence presented here justifies the close linking of those two names. Roys's system of whaling did not work. The authors attribute his failure to character defects, poor leadership, and careless organization; but surely his fundamental error was his singleminded pursuit of a system based on rockets and whaleboats (already tried and rejected in Britain) when the future lay with harpoon guns and

steam catchers. In assessing Roys's role as "America's Pioneer of Modern Whaling," some consideration of those—such as the Williams family—who were using explosive gun harpoons and, eventually, steam catchers would have been useful. So, too, would a consideration of the whaling context. Rockets were hardly necessary for securing whalebone, which was soon the major source of revenue, and there is no hard evidence that they could catch rorquals. In any case, falling oil prices delayed the hunt for rorquals until after Foyn's methods had triumphed.

The fact that Roys was a failure does not detract from the value of this study of a crucial period of whaling between the traditional and modern phases. Even if Roys could not make the transition, this account of his attempts to do so is a very illuminating survey of the problems involved and of one man's attempts to solve them.

GORDON JACKSON  
University of Strathclyde

ROXANNE DUNBAR ORTIZ. *Roots of Resistance: Land Tenure in New Mexico, 1680–1980*. (Chicano Studies Research Center Publication, number 10.) Los Angeles: Chicano Studies Research Center and American Indian Studies Center, University of California, Los Angeles. 1980. Pp. vi, 202. Cloth \$14.95, paper \$9.95.

The conquest of New Mexico is not a subject that has elicited adequate response from older historians who have chosen to treat it generally as a natural result of the territorial expansion of the nation. Many have preferred to gloss over the military seizure and subsequent colonization of the region, while others have offered a number of lamentable excuses for such imperialistic behavior. Until recent times, however, the calamitous consequences of the hard fact that we overran and seized peoples of different cultural backgrounds were ignored or forgotten or both by most historians of the Southwest, forming a curious sin of omission in Southwestern historiography. There was a failure to point out that we wanted only the lands of those peoples we hoped would disappear or, like the Indians, move to other areas. The descendants of the affected populations, the Mexican-Americans and the Pueblo Indians, who were, and remain, the victims, have not forgotten. Neither have other emerging historians, such as the author of this book, Roxanne Dunbar Ortiz, a socially conscious historian. In this well-written and equally well-researched socioeconomic study, she shows how in the postconquest period the introduction of the capitalist system drew New Mexico into the national economy. The impact on the natives, however, was largely negative.



They were now dependent on a new economy that many did not understand and often dispossessed of their communal and private landholdings as a result of their ignorance and of Anglo legal chicanery. Nevertheless, the Pueblo Indians and the Mexican-Americans have each retained their sense of collective cultural identity. They have also maintained to a great extent many of the socioeconomic relations growing out of their traditions of common controls of certain lands and water distribution in their quests for survival.

One of the more intriguing questions posed in Ortiz's study, as it has been in the past by scholars such as Albert Schroeder, is why or how the unique institutions survived. Was it because their social structures had enough in common with the Spanish cultural institutions to permit them to accept only a minimum of Hispanic ideas while retaining their basic values? Or did the Pueblo and Mexican-American cultures survive because the Spanish government was indifferent to, or incapable of, enforcing the processes of Hispanicization of the native institutions? Perhaps the question is a moot one because, as Ortiz explains, they did preserve their collective identities in the face of the Hispanic and Anglo invasions.

This work, which includes extensive documentation and an excellent bibliography, is a gripping story of ruthless exploitation that, along with a handful of other works beginning with the pioneer efforts of Carey McWilliams, enables one to develop a better understanding of the present-day condition of Southwestern society. It helps provide the historiographical foundation on which all future historians of the subject will have to build.

ALVIN R. SUNSERI

*University of Northern Iowa*

LEWIS O. SAUM. *The Popular Mood of Pre-Civil War America*. (Contributions in American Studies, number 46.) Westport, Conn.: Greenwood Press. 1980. Pp. xxiv, 336. \$29.95.

This book exhibits and interprets testimony from plain Americans between 1830 and 1860 about their leading concerns. It neither skimps data nor pounds conclusions. It selects testimonies mainly from letters and diaries, manuscript and printed. Popular attitudes were traditional concerning "Things Spiritual" (providence, religion, conversion-revival-millennium, death) and "Things Temporal" (self and society, politics and the nation, nature and art, the West). To a learned, controlled, judicious, thoughtful study a nimble intellect and graceful pen lend charm. That Saum knows the pitfalls and pratfalls of his genre he makes clear in a

fine appendix on his methods of study. The pratfalls he has studiously avoided. Perhaps some pitfalls are inevitable.

Beyond correcting our understanding of the popular mood of the era, the book raises important questions about studying plain people's concerns. Saum selected manuscripts from collections from Maine to California, and his printed sources are of wide provenience. But how widespread and numerous must testimonies be to represent "the popular mood" of a country? Granted that Saum's investigations have been thorough, at what point does thoroughness rise beyond the anecdotal to what was actually popular?

Wisely is Saum wary of the notion that dicta by sophisticates tell the inarticulate majority's moods. Repeatedly he shows that perspectives on life announced by Emerson anticipated or shaped later attitudes more than they reflected contemporary concerns. But that the other oft-cited literary sources are Hawthorne, Melville, Webster's Dictionary, and Mark Twain does seem arbitrary. Twain is of a later period, and do fictions and definitions best test the autonomy of popular moods? The main testifiers are carefully chosen for having been followers and not leaders. Relatively uneducated and powerless they were, but they still constituted an articulate minority within the inarticulate masses for whom they are taken to speak; they not only could write, they *did* write letters and diaries. In doing so they recorded remarkably consistent opinions, often in identical phrases. Do we have here common people writing what they felt and thought, or what they felt and thought they ought to feel and think? Letters and diaries by other than poets are notably formulaic genres, reflecting the copybooks, sermons, speeches, stories, and other media of cant and cliché employed by opinion makers. The widespread, conservative consensus that Saum documents is valuable knowledge in its own right, but must not its occurrence be explained if we are to grasp its significance?

Such queries aside, the book's nonquantified, somewhat miscellaneous evidence provides a fresh view of the period, proving that these testimonies sometimes support and sometimes undercut what the period's literati and the recent historiographers said it was like there and then. Thus the book fully deserves a place in the distinguished series it occupies. It raises a host of issues about popular history, and it answers many of them. Regrettably, it is peppered with typos.

WILLIAM A. CLEBSCH  
*Stanford University*

LEWIS PERRY and MICHAEL FELLMAN, editors. *Antislavery Reconsidered: New Perspectives on the Abolitionists*. Baton

Rouge: Louisiana State University Press. 1979. Pp. xvi, 348. \$20.00.

Fifteen years ago Martin Duberman edited *The Antislavery Vanguard: New Essays on the Abolitionists*. Lewis Perry and Michael Fellman have prepared a comparable collection of abolitionist scholarship by notable younger historians. The editors and essayists strive to be gracious to their predecessors but too often condescendingly characterize the Duberman volume and his generation of historians as so desiring to be relevant to civil rights issues of the 1960s that objective history and imaginative historical concepts were slighted. Neither Duberman nor Howard Zinn might object to being accused of having responded to demands for racial relevance; *The Antislavery Vanguard*, however, also presented the pioneering and still enduring work of David Davis, Leon Litwack, James McPherson, Benjamin Quarles, Willie Rose, John Thomas, and Robin Winks.

Perry and Fellman suggest that the "morbid absorption in their own internal history" (p. x) by abolitionists, and repeated by past abolitionist scholars, also caused neglect of "grand themes" (p. xii). The strongest essay is James Stewart's perceptive analysis of Wendell Phillips's life and personality: his battle, as well as society's, between control and expressiveness. Noteworthy is Joseph Glickstein's discussion of why abolitionists's tolerated poverty but not slavery. Allen Kraut's quest for the social origins of Liberty party supporters (many "laborers and mechanics") deserves attention for its author's statistical efforts as well as his explicit modesty, given the small sample available. Fellman's analysis of "Bleeding Kansas" diatribes convincingly shows the conflicting class nature of North-South rhetoric. As always, Ronald Walters provokes thought in seeking a cultural perspective. But we need some indication about what has happened to the argument he presented in *The Antislavery Appeal* (1976) that evangelical Protestantism should disappear as a significant theme of abolitionist scholarship. It remains very significant in the Perry and Fellman volume.

*Antislavery Reconsidered*, unlike Duberman's volume, largely neglects black abolitionists and white racism except for a diffuse essay by Carol George about the black church, and a lucid one by Leonard Richards on the proslavery sympathies of Jacksonians. Blanche Hersh and Ellen DuBois, on the other hand, reflect recent discussions in women's history and its relationship to abolitionism. Unfortunately, given the opportunities provided by Perry and Fellman for "grand themes," Hersh merely "adapted" chapter 1 of her book and DuBois summarized one quarter of her's.

Failing to fulfill the editors' grandiose prediction that abolitionist history is being "fundamentally al-

tered" (p. xiv), *Antislavery Reconsidered* nevertheless merits praise for aspiring to avoid some past approaches. Ironically, future historians may view this volume—with its emphasis on evangelical Protestantism and white popular culture—as mirroring excessive presentism: current religious revivals, white backlash, and their political impact.

TILDEN G. EDELSTEIN  
Rutgers University

JOHN MAYFIELD. *Rehearsal for Republicanism: Free Soil and the Politics of Antislavery*. (National University Publications Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1980. Pp. 220. \$17.50.

In 1849 Charles Sumner believed that the Free Soil Party might soon control Massachusetts and that, given its "talent, principle and sincere devotion to the public good," Free Soilism would "leaven the whole lump" of American politics. While Sumner had personal reasons for his expectations (he would soon be elected to the United States Senate by a coalition of Free Soilers and Democrats), many Northerners shared his enthusiasm for a new political movement opposed to the extension of slavery. Not only did the Free Soil ticket receive 12 percent of the vote in 1848, but also the party sent 10 congressmen to Washington, and countless legislators represented its principles in state affairs. But by 1851 Sumner's optimism had dimmed, and the party's poor showing a year later as well as its failure to maintain a broker's position in Massachusetts, Ohio, and New York spelled the end. "The work," Sumner concluded, "is to be done over."

John Mayfield's *Rehearsal for Republicanism: Free Soil and the Politics of Antislavery* retells this story. Despite its inappropriate title (the monograph deals with Free Soilism on its own terms, not as a precursor to the Republicans), Mayfield meticulously charts "the personalities, platforms and politicking" of this Northern-based third party. By shaping his monograph around the ideology of the movement and the political environment within which it operated, Mayfield conveys the sense of an organization dedicated to the overarching principle of denationalizing slavery, while simultaneously constrained by internal factions, external coalitions, and the necessitous compromises of electioneering. Mayfield's most important contribution rests in his perceptive unraveling of the ideological roots of the three groups that created the Free Soil federation—the Barnburners, Conscience Whigs, and Liberty party.

Apart from the discussion of the roots of Free Soilism and the explanation of the party's decline, there is little new in Mayfield's study. Given his traditional approach to political history, the mono-

graph is an event-filled, from-the-top-down, institutional history that sometimes reduces Free Soilism to a conversation among its leaders. While Mayfield believes ideology is a good way of "peering into the ordinary voter's mind," it is never clear who these voters were. And Mayfield's primitive use of statistics does not help.

Rather, the historical service of this briskly written monograph is to bring together the organizational facts of the Free Soil Party's career, to reassemble them into an incisive narrative, and to provide a comprehensive look at a party once called "the unbidden guest" of mid-century politics.

JEAN H. BAKER  
Goucher College

STEPHEN Z. STARR. *The Union Cavalry in the Civil War. Volume 1, From Fort Sumter to Gettysburg, 1861-1863.* Baton Rouge: Louisiana State University Press. 1979. Pp. xiv, 507. \$27.50.

In this first of three volumes, Stephen Z. Starr narrates the history of the Union cavalry from inception to Gettysburg. Developments in the West, excluded after the 1861 cavalry organization, presumably will be treated in the second volume. The book opens with General James H. Wilson's 1865 Selma campaign in which cavalry was used not merely for a raid of peripheral significance but as an independent army with a major mission to destroy rebel resources in Alabama. Wilson documented that cavalry, properly concentrated, equipped, trained, and led, was devastatingly effective.

This opening is well chosen, for it underlines the central thesis of the volume: until late in the war, Union commanders failed to appreciate the cavalry's potential and this, compounded with inexperience and inadequate equipment, produced poor performance. Prior to June 1863, cavalry was a minor factor in most campaigns: at Fredericksburg the mounted arm suffered only eight casualties and, frequently, units were worn out on orderly details and extensive picketing. (The Confederates used infantry pickets, allowing cavalry concentration into a single group.)

Starr suggests that Joseph Hooker was primary in beginning improvement by consolidating the cavalry and inspiring the men. New confidence and competence were seen at Brandy Station and in the Aldie-Middleburg-Upperville chain of battles fought in June 1863. The cavalry encounters at Gettysburg, largely successful, underlined that a new phase was approaching, and the author sees this period as a watershed between the old and the new. Problems remained, particularly in inept leadership by Alfred Pleasonton and others, but the

cavalry now held its own and increasingly would be better equipped and mounted than its opponent.

This well-written book is the first recently to attempt a complete narrative history of the cavalry. Some fascinating material is presented, particularly on the problems of a volunteer army with a high percentage of sloth and drunkenness among junior officers. But a weakness is that the book relies too heavily on unit histories and slights contemporary letters and diaries. Also, most secondary sources published in the last decade are missing, which may account for the uncritical acceptance of old assertions, never properly documented, such as the claim that Southerners rode better and were more warlike than Northerners. Like most accounts that concentrate on one side of the North-South issue, the author's generalization about the South lacks depth. No truly adequate explanation of the Union's failure to properly organize cavalry is given: the argument that Southerners better appreciated horseflesh is unsatisfactory as Winfield Scott, who depreciated use of the mounted arm, was a Virginian.

In its major conclusions the book does not go very far beyond the work of Fairfax Downey and Ray Stoney, though one expects that future volumes, dealing with more neglected areas, will yield fresh insights. The work will be well received by buffs and would add to research collections on the war.

MICHAEL C. C. ADAMS  
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RICHARD J. SOMMERS. *Richmond Redeemed: The Siege at Petersburg.* Garden City, N.Y.: Doubleday. 1981. Pp. xxii, 670. \$22.50.

The siege of Richmond and Petersburg was the longest single campaign of the Civil War. For nine months Federal forces under General U. S. Grant slowly tightened a stranglehold around the two principal Virginia cities that still remained defiant. Grant was not content merely to sit back and starve the Confederate defenders into submission. Seven times in the summer and autumn of 1864 the Federal commander made slashing attacks on General Robert E. Lee's thinly manned defenses. This book, its sweeping title notwithstanding, treats only of the heretofore neglected fifth offensive.

For four days (September 29-October 2, 1864), Union forces made assaults on two fronts. One sector was along the north bank of the James River, with attacks there intended as a direct stab at Richmond; other Federal assaults were launched west of Petersburg and were designed to snap the all-important Southside Railroad, Lee's lifeline to the west. As at Chancellorsville, Lee was expected to be caught in the middle and outflanked, if not destroyed. In the end, Sommers states here, the two-

pronged offensive "brought the Yankees no great victories, but it did enable them to overrun three forward Confederate lines, to conquer strategic sectors on both sides of the James, and to tighten their grip on both sides" (p. 419). The chief prize for the Federals was Fort Harrison. Combined casualties exceeded 9,300 men.

Richard J. Sommers's book evolved from his Rice University dissertation, which has to rank as one of the largest compilations ever produced for a graduate degree. There can be no question but that here is the definitive history of the fifth offensive in the siege of Richmond, for this is a minute-by-minute, unit-by-unit, blow-by-blow account of everything pertaining to that 100-hour campaign. Each general receives proper introduction; corps, divisions, brigades—sometimes even regiments and batteries—are ushered individually into action; collisions between pickets, skirmishes, and major fighting at Chaffin's Bluff and Poplar Grove Church are covered in the most minute detail possible. Excellent maps guide the reader through Sommers's probing narrative. The sheer depth of his presentation is apparent in 90 pages of notes and a bibliography that requires 38 pages to itemize.

In addition to giving a painstakingly complete story, Sommers is also commendably detached in viewpoint. He has no axe to grind or bias to flaunt. He focuses more on the Federal side, but one suspects this was done because of a preponderance of Northern source material plus the fact that the Union side was calling the shots throughout the campaign. Certainly Sommers's many interpretations and conclusions are exceptionally fair for military activities so filled with action and might-have-beens.

The time span of this work is quite limited, to be sure, but the exhaustive treatment of the subject is such that it doubtless will become more of a reference work than a volume for mere popular consumption.

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ERIC FONER. *Politics and Ideology in the Age of the Civil War*. New York: Oxford University Press. 1980. Pp. 250. \$15.95.

These seven essays (all but one previously published) explore the driving impulses of the Civil War era. In dealing with the historiography and causes of secession, ambiguities in antislavery thought, Reconstruction, and radical labor movements, Eric Foner is fully committed to the ideological approach to American political history. He emphasizes the importance of coherent, unified belief systems as both causes of events and weapons of po-

litical conflict. What unites the period from the 1840s to the 1870s in Foner's subtle analysis is the emergence of a dominant Northern ideology as the connecting tissue among class, economic, sectional, and political concerns. The ideology concerned the role of labor in a capitalist society. It was a synthesis between the dominant bourgeois liberal ideal of the free individual and a more radical vision, espoused by some abolitionists and labor leaders, that stressed individual economic independence but also incorporated a fundamental critique of a society rooted in competitive individualism. The Republican party represented the synthesis and successfully popularized it as both anti-Southernism and a quest for individual freedom. The more radical vision survived among some labor leaders and a few politicians such as Thaddeus Stevens (who is the subject of one of the essays).

Foner is excellent at delineating the dominant ideologies and linking them to political events. Still, some problems exist in his approach. His interest centers on the dialogue among different strands of the sectional power elite of the Republican party, which he believes dominated the North. But there were other important belief systems in the North. Democrats were hostile to the transforming ideas Foner emphasizes. They were numerous, powerful, and influential. Their role and importance and the complexities they caused are not important to Foner—Democrats rarely appear (except for the antislavery-extension Barnburners) and certainly are not included as part of the dialectic. But the Northern Democrats cannot be ignored, as the Republicans of the time realized. Foner is also hostile to the work of those political historians who quantify. He does not find their conclusions useful or persuasive, especially those concerning ethnocultural conflict. Yet those findings are an essential part of any Northern ideology, and they impose limits and boundaries and a different focus on Foner's emphases.

Finally, as in other studies emphasizing ideologies as the engines of political conflict, Foner does not excel at the transition game: showing how forces are aroused, grow potent, and are resisted. Class consciousness had to be awakened; partisan political consciousness had to be transformed. It is an important process that remains to be understood, especially given recent work on mass politics generally and the Northern Democracy more specifically. Foner recognizes the early importance of intersectional political parties in resisting and containing sectional confrontation, but he emphasizes their demise in the face of popular sectional ideologies. Much more remains to be considered about that process.

This is an important and invigorating book—but not because it settles many questions. Rather, in suggesting a particular focus and understanding, it



helps set an agenda that should have as its centerpiece a dialectic between the two new political histories, a dialectic that ultimately should produce a more complete, complex, and satisfying synthesis, involving winners and losers, critical elites and politicized masses, the labor problem, and ethnic tensions.

JOEL H. SILBEY  
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JACQUELINE JONES. *Soldiers of Light and Love: Northern Teachers and Georgia Blacks, 1865-1873*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1980. Pp. xiii, 273. \$17.50.

Jacqueline Jones's gracefully written and forcefully argued case study of early Southern black education uses Georgia to explore the freedmen's teachers, their sponsors, the freedmen's response to Northern-initiated schooling, and the contradictions in the education movement. *Soldiers of Light and Love* is valuable as an investigation of the tensions between Northern white and Southern black expectations and demands and of the rancor that often developed as a result. The volume also provides a sensitive interpretation of the uses Northern white female teachers sought to wring from freedmen's education. While not persuasive in her assertion that the teachers were abolitionists, she successfully argues that although these women were trapped within an oppressive ideology of womanhood, they used that ideology to achieve at least a measure of personal independence.

While Jones provides insightful interpretations, her volume suffers from a number of erroneous assertions and misstatements of fact, arising in part from her selection of Georgia as a test case. Throughout the eight years of her study, the American Missionary Association (AMA) dominated freedmen's education in Georgia to an extent unequalled elsewhere. Because of that dominance, Jones relies heavily on the association for all of her explanations. The freedmen's education movement was not simply the AMA writ large, however. Indeed, several of the practices of the AMA from which she generalizes were peculiar to the AMA and a few kindred groups only. Two examples must suffice here. First, contrary to Jones, several organizations had equalized salaries for men and women teachers by 1866. The AMA and others never did. Second, not all associations adopted the male-dominated bureaucratic form as did the AMA. In that body it was true that women "enjoyed little opportunity to make policy decisions" (p. 97). In contrast, however, other groups operated through committees rather than hierarchical bureaucracies, and

women served on many of the committees, often wielding significant power over policies and operations. Thus, while several of Jones's conclusions are accurate for the AMA, they do not hold for the aid movement generally.

Equally troubling are simple inaccuracies. At least ten organizations sponsored Northern teachers in Georgia rather than six. Her enumeration of AMA teachers is accurate, but she seriously undercounts the teachers in rival groups. Among other examples: the Methodists began their educational work in 1866, not 1864; her claim of \$15 million expended on freedmen's education between 1865 and 1870 is at least \$3 million too high; and the American Freedmen's Union Commission was based in New York City, not Washington, D.C.

Used carefully, this study will contribute significantly to the social history of teaching, to women's history, and to the history of American race relations. As a history of the freedmen's education movement generally it is too often unreliable; as a history of teachers in freedmen's schools it is unexcelled.

RONALD E. BUTCHART  
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Cortland

RONALD E. BUTCHART. *Northern Schools, Southern Blacks, and Reconstruction: Freedmen's Education, 1862-1875*. (Contributions in American History, number 87.) Westport, Conn.: Greenwood Press. 1980. Pp. xiv, 309. \$25.00.

*Northern Schools, Southern Blacks, and Reconstruction* is a disappointing work. Examining the preface, the reader begins wondering if the author has command of the subject. The stated purpose behind this book is to provide a detailed intellectual and social history of the origins of Southern black education, put freedmen's schools into the context of Reconstruction and black liberation, and analyze "the ideas of the northerners who organized freedmen's education, noting the contradictions in ideology that emerged among the educators and the contrasting vision of Afro-Americans and the society that informed their ideas" (p. xi-xii). Since Butchart has evidently not made up his mind about what he is trying to do, it is understandable that his writing is unclear.

The bulk of the study centers on what Butchart contends are two distinct educational ideologies: the secular and the evangelical. Under the category of secular, he places the freedmen's aid societies and the Freedmen's Bureau. Under evangelical, he focuses on the American Missionary Association and the American Freedmen's Union Commission. According to the author, the freedmen's aid societies held an egalitarian view of education for blacks and

maintained that "the freedmen were to be active participants in the republic on an equal footing with all other citizens" (p. 15). The AMA and AFUC, he argues, prescribed a limited brand of schooling for blacks, one geared toward making them obedient and docile; "the denominations were more concerned with stability and the rationalization of control and discipline than with extending the boundaries of black freedom" (p. 52).

The real question is whether the educational efforts of the evangelicals or secularists aided the cause of black freedom and autonomy. The point is not the degree to which the aid societies differed from their missionary brethren on the issue of black education but the extent to which they were in agreement. Contrary to Butchart's argument, religion was central to the educational philosophy and program of the freedmen's aid societies. Racism and white hegemony were at the core of both groups, and this translated into an education that worked to thwart black freedom. Butchart should have given careful attention to the works of Carter G. Woodson, especially his *Miseducation of the Negro*, and the writings of George M. Frederickson, William S. McFeeley, the published and unpublished work of Robert F. Engs, and others.

The author overlooked one study that could have helped him immensely in developing and sharpening his thesis. Butchart's concern is with black public schooling. Nevertheless, he makes no mention of the single most important book on this subject: William Preston Vaughn's *Schools For All: The Blacks and Public Education in the South, 1865-1877* (1974), which was the recipient of the Phi Alpha Theta award.

Butchart's book is disappointing in that it had the potential of being much more. The ingredients are there. The author evidently devoted a fair amount of time to this study. He explores and cites a massive amount of primary materials, and he conveys valuable nuggets of information here and there. His contrast of the curriculum goals of the Northern educators and the expectations and aspirations of the freedmen is important. But here, too, the author neglects the larger historical questions of, for example, traditional African education and its retention and transmission through the non-traditional schooling blacks maintained in the South. The studies of Vaughn, Thomas Webber, and Abdou Moumouni are very helpful in this regard. My suspicion is that Butchart's training and real expertise are in educational studies rather than history, although the vita at the conclusion of his book does not indicate his academic discipline. I have reviewed his book as a historian. Perhaps his work will receive higher regard among the education people.

DONALD SPIVEY  
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ERIC ANDERSON. *Race and Politics in North Carolina, 1872-1901: The Black Second*. Baton Rouge: Louisiana State University Press. 1981. Pp. xiv, 372. Cloth \$30.00, paper \$12.95.

Eric Anderson introduces his study of North Carolina's second congressional district from 1872 to 1901 with a reference to C. Vann Woodward's suggestion that there had been "forgotten alternatives" in American race relations between the end of radical Reconstruction and the turn of the century. Although Anderson focuses on local politics, his book is relevant to Woodward's thesis and to some broader questions about race relations in the American South of that time. Clearly the black second was special, but, as Anderson points out, it was not unique in its election of blacks to Congress after 1878, in its continuing Republican strength, and in its ultimate transformation by the triumph of racism around 1900. As drawn by the Redeemers in 1872, the second was a masterful gerrymander that extended from Craven County in the southeast to Warren and Northampton Counties along the Virginia border. By concentrating these largely black counties in one district, the Democrats protected themselves in the surrounding counties while tacitly acknowledging that the second would be a Republican and black stronghold. In such an area a policy of racial accommodation was practiced as a necessity, and with some interesting results. Republicans, including four blacks, won the congressional seat in all but two elections between 1872 and 1898. Similarly, Republicans, also including many blacks, controlled many of the local offices and regularly sent representatives to the state legislature.

Changes were foreshadowed, however, with the campaign of 1888 when the Democrats invoked racial fears as a means of countering the Farmers' Alliance and an attractive Republican gubernatorial candidate. Thereafter, economic distress prompted an emigration of many blacks from the district, and in 1891 a drastic redrawing of the district's boundaries further reduced its black population. Internal discord in the Republican party added to the problems until black and Republican strength in the second largely collapsed. In the mid-1890s Republican-Populist fusion tactics brought a temporary revival and resulted in the election to Congress in 1896 and 1898 of George H. White, the last black to sit in Congress in the post-Reconstruction era. But for blacks the decade ended in tragedy. Democrats, determined to rid the state once and for all of black political power, conducted a massive white-supremacy campaign in 1898 and regained control of the state legislature. They then passed a disfranchising literacy test amendment, which was approved by the voters in August 1900. Accommodation had succumbed to racism.

Anderson's well-researched book, which began as a doctoral dissertation under John Hope Franklin at the University of Chicago, provides a wealth of data on racial politics in the second district for the years in question. Unfortunately, this very detail becomes a problem, particularly in chapters 2 through 11, where his rather mechanical review of one election after another is likely to try the endurance of readers not devoted to the minutiae of North Carolina local politics. But from chapter 12 until the end, with his account of the fusion tactics, the white supremacy campaign, and his conclusions, the pace significantly improves. Anderson's portrayal of George H. White, a central figure in the final years of his story, is sensitive and sympathetic, in contrast with the views of some earlier historians, and his analysis of the changes in white attitudes toward blacks between 1880 and 1900 is convincing. As he clearly shows, race relations were not static; what was politically acceptable in 1880 became impossible in 1900.

In his conclusions Anderson observes that "white supremacists turned progress into a trap, using black success to stir up race hatred and depicting the Democratic party as the only home for 'true' white men" (p. 335). This was indeed a tragic ending to his story. But it was not unique. Thus Anderson is correct in asserting that the history of the second district has more than local interest. Readers willing to wade through some tedious accounts of local elections will find that in the end Anderson has made a worthwhile contribution to our understanding of race and politics in the post-Reconstruction era.

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PAUL STUART. *The Indian Office: Growth and Development of an American Institution, 1865-1900*. (Studies in American History and Culture, number 12.) Ann Arbor: UMI Research Press. 1979. Pp. xvi, 243. \$27.95.

This book by Paul Stuart is the most detailed study to date of the administrative development of the Indian Office in the critical period between 1865 and 1900. Its theme is "institutionalization," its thesis that centralization made institutionalization possible. Drawing its main ideas and organizational framework from formal organization theory, particularly the work of Philip Selznick, the study uses records of the secretary of the interior, the BIA, the Civil Service Commission, congressional and executive department documents, writings from the period, and recent works on Indian affairs.

Three major "indicators" of institutionalization are examined: personnel selection (Indian agents, agency personnel, and civil service), coordination of

internal procedures (Board of Indian Commissioners, superintendents, and inspectors), and goal definition (U.S. Indian policy and the increasing share of education in the Indian Office budget). Education is viewed as a focus for centralization and a means for accomplishing Indian assimilation. Stuart builds a persuasive case for his centralization thesis but not for his extrapolations from it. By 1900 institutionalization was incomplete, but "the direction which organizational development would take was not in doubt" (p. 159). The author believes that for the next seventy-five years the task of the Indian Office would be conceived as "primarily educational," an assessment that would surprise the authors of the Meriam Report who in the late 1920s urged "recognition" of the Office's work as "primarily educational." Citing organizational theory, Stuart asserts that "the development of administrative capacity by 1900" coupled with "premature consensus on the means" for accomplishing assimilation contributed to the "failure" of the Indian New Deal (p. 159). Unfortunately, these points are not further developed.

Reliance on organization theory enables the author to reveal significant administrative trends in institutionalizing the Indian Office by centralizing it. This explication constitutes the book's main contribution. The theory proves to have little explanatory power with reference to two pillars of Dawes era policy, allotment and citizenship, because the author considers that they had minor direct administrative impact. The third pillar, education, is treated more extensively but again from an administrative viewpoint. The nature of Indian education, its effect on "clients" (Indians remain either offstage or only dimly visible), and its relationship to other forces in American society, educational and otherwise, are dealt with in cursory fashion. The reader gains a valuable but narrowly based analysis of bureaucratic development, full of administrative dynamics but singularly lacking in social or cultural ones. The author's use of organization theory is thus both the major strength and the major weakness of the book.

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RAY ALLEN BILLINGTON. *Land of Savagery, Land of Promise: The European Image of the American Frontier in the Nineteenth Century*. New York: W. W. Norton. 1981. Pp. xv, 364. \$18.95.

With the help of research assistants and translators supported by a grant from the National Endowment for the Humanities and volunteer aid from senior colleagues in several European countries, Ray Allen Billington has surveyed the depiction of

the American frontier in an immense body of novels and nonfictional documents in a dozen languages. A twenty-page bibliographical essay lists and discusses secondary scholarship in almost as many languages. It was a formidable undertaking that required both administrative and scholarly skills, and Billington deserves the thanks of the profession for his thoroughness, as well as for his clear, unpedantic style.

The study devotes much space to imaginative literature because of the international appeal of stories about adventure in the Wild West. From Fenimore Cooper's *Leatherstocking Tales* to the present, novels and stories of the frontier by American authors have been translated and widely read in Europe. Furthermore, a number of European writers have produced a subgenre of original "westerns"—analogous to the "spaghetti western" films made in Italy in our own day. The outstanding example, the German Karl May (1842–1912), said to have been the favorite author of both Adolf Hitler and Albert Einstein (p. 56), wrote more than seventy novels laid in the American West. His books were translated into twenty languages and sold more than thirty million copies; a collected edition of his works in seventy volumes was published as recently as 1949–65. Other European novelists have also been prolific and highly successful in producing westerns, including the German Balduin Möllhausen, the Englishman Mayne Reid, and Gustave Aimard in France. There have even been Polish and Hungarian westerns. These stories presented both noble and treacherous Indians and both heroes and desperadoes among the white scouts, hunters, Indian fighters, and ultimately cowboys who peopled an often unrecognizable West. (George A. Henty has characters make a journey entirely by boat from San Diego to Santa Fe.) But in any case the "persisting fictionalized image of the frontier" (p. 320) has featured violent combat.

The agrarian West, the land of promise, has of course been much less attractive as material for fiction. Instead, it gave rise to an avalanche of travelers' accounts, promotional tracts, guide books, and "America letters" in which the hardships and satisfactions that emigrants might expect in the New World were described and debated. The contrasting images often had political implications: landlords and employers wanted to discourage emigration in the belief that it tended to raise wages by decreasing the labor supply, whereas liberals and radicals encouraged emigration for the same reason. The cultural backwardness of frontier communities was generally recognized, but the depiction of opportunity and equality in the American West "helped break the mold of tradition that had slowed change for generations" in Europe (p. 309).

Billington emphasizes the contradictions in his widely varied sources and is cautious about drawing

conclusions. He does assert, however, that "an alarming number of people" have been led by both fictional and nonfictional writing about the American frontier—with recent encouragement from "the propaganda of Communist countries"—to see the United States as "a ruthless predator, a foe of minorities, and an enemy of progress" (p. 382).

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MARK P. LEONE. *Roots of Modern Mormonism*. Cambridge: Harvard University Press. 1979. Pp. ix, 250. \$15.00.

Between 1890 and 1907 the Mormon church abandoned polygamy, dissolved its own political party, and divested itself of control over Utah's economy in order to become a full participant in the American union. Mormon historians Leonard Arrington and Davis Bitton in an important book, *The Mormon Experience* (1979), minimize this shift in Mormonism, characterizing it as "creative adjustment." For non-Mormon Mark P. Leone, a University of Maryland anthropologist, the change was more dramatic. The question he seems to be asking in this interesting, fresh study of Mormonism is: how did a radical nineteenth-century American utopia—espousing common ownership of property and classlessness—become a conservative, business-oriented, international church in the twentieth century?

Mormonism is usually characterized as a hierarchical, authoritarian church with a fixed theology. But Leone sees this as only the surface, not the core (p. 171). Using late-nineteenth-century primary materials from the Mormon settlements in eastern Arizona, contemporary secondary works, and personal visits to Mormon wards in Arizona and Utah, he concludes that Mormon thought is very democratic and flexible because it is individualized, each Mormon working out the meaning of a practical religion by applying the abstract principles of the faith to daily life. Leone asserts that these individual applications are not subject to criticism by the church, partly because it lacks theological or historical sophistication. Since the practical problems of life are constantly changing, Mormonism's "do-it-yourself" theology is flexible. Unlike nineteenth-century Mormonism, it fails to provide a critique of society, despite their self-identification as a "peculiar people." Symbols of peculiarity—like the recent denial of priesthood to blacks—substitute for the real thing. "Mormonism has produced a population which directs itself according to society's ends while hiding that fact from itself," concludes the author (p. 223). He suggests a new symbol may be found in opposition to the ERA, abortion, working women, or gay rights.

*Roots of Modern Mormonism* is a useful counterpoint



to the traditional understanding of Mormonism, but I suspect the author carries his generalizations too far in the other direction. To be convinced that the church is an "intellectual democracy" (p. 190), I would need to know whether in church meetings Mormons are really free to express ideas at variance with the multitudinous doctrinal principles contained in the *Book of Mormon* and other writings of the Mormon prophets. How does Leone deal with the familiar Mormon statement, "When the prophet speaks, the debate is ended"?

It appears that Leone has sampled but not immersed himself in the primary and secondary sources. The reader may also question whether his personal visits to Mormon wards were sufficiently numerous or representative. A number of minor historical errors occur that add to the suspicion that the author has come to many of his generalizations too easily. He incorrectly dates the alleged migration of the *Book of Mormon* people from Jerusalem to the New World (p. 12), the second edition of Fawn Brodie's *No Man Knows My History* (pp. 29, 242), a movement of Mormons from Ohio to Jackson County, Missouri (p. 14), and seems to have the horde of European converts arriving in the United States too early (p. 14). He gives the wrong name for the church organized in 1830 (p. 11), fails to use the first edition of Joseph Smith's revelations when the revelation quoted was substantially changed in later editions (p. 13), and thinks the Reorganized L.D.S. Church is the only mid-nineteenth-century Mormon splinter group that has survived (p. 17). The reader might also challenge his suggestion that Mormonism contains important Calvinistic elements (p. 49), that the "destruction of Mormonism was inevitable as soon as the Civil War was over" (p. 26), or that there are few active Mormon historians (p. 205). The reader may also wonder about the "centrality of women" (p. 41) in a religion that "calls all members priests" (p. 104), when in fact women are not ordained, and may have expected the author to specify the Supreme Court decision that supposedly asserted that "none of the provisions of the Constitution applied to United States territories" (p. 150).

Nevertheless, the author has produced a valuable work that will provide a needed corrective to previous works and should be the basis for further research testing his provocative generalizations.

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DONALD W. CURL. *Murat Halstead and the Cincinnati Commercial*. (A Florida Atlantic University Book.) Boca Raton: University Presses of Florida. 1980. Pp. ix, 186. \$17.50.

Murat Halstead gained national repute as a reporter with his accounts of the political conventions

of 1860. Through the Civil War, his dispatches to the Cincinnati *Commercial* were widely republished. In the Reconstruction era, Halstead advocated "independent journalism"—an approach that separated the press from its patrons and required that revenue derive only from paid advertisements explicitly printed as such. Further, as an independent, Halstead believed he could be partisan but that partisanship did not require unswerving loyalty to party or to individual politicians.

Together with other independent editors and publishers, Halstead's support became an eagerly solicited force in the struggles for Liberal Republicanism through the 1870s. Halstead attended and reported on every national Republican convention from 1860 through 1904, and his views were widely known, if not always respected.

As Donald W. Curl shows in this brief analysis, Halstead's success, both as a journalist and as a politician, was always marred by rash judgment, an acid pen, lasting enmities, and a series of grave political mistakes. Afflicted by this *hubris*, Halstead's career from the early 1880s through his death in 1908 reflected a decline from the high ideals of independent journalism, as well as a decline in personal fortune and reputation.

Curl's work provides several insights into the transition to sensational journalism that preceded the emergence of Joseph Pulitzer and William Randolph Hearst in the last decades of the nineteenth century and the muckrakers of the early twentieth century. Through a selected group of Halstead's *exposés*, Curl gives a readable and concise sketch of some of the political intricacies of the 1870s and 1880s. The work does not exhaustively explore all of the positions of the *Commercial* during Halstead's period, nor does it shed much light on the complexities of Halstead's obviously troubled character. Since the author intended neither a definitive journalistic history nor a psychobiography of Halstead himself he can hardly be faulted for choosing a format that allows for a lucid and close focus on a few outstanding controversies to illustrate the journalist's rise and fall.

Although limited by a lack of sources on the personal side of Halstead's life, Curl demonstrates what a solid contribution can be made in their absence. As a study of the public side of a leading political journalist, the book strengthens our knowledge of both politics and journalism through the late nineteenth century and gives us the first scholarly treatment of this figure, important in both fields.

RODNEY CARLISLE  
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LOUIS J. KERN. *An Ordered Love: Sex Roles and Sexuality in Victorian Utopias—the Shakers, the Mormons, and the Oneida Community*. Chapel Hill: University of North

Carolina Press. 1981. Pp. xiii, 430. Cloth \$24.00, paper \$12.50.

A brief review cannot do justice to Louis J. Kern's detailed analysis of the sexual ideology and behavior in three Victorian communal experiments: Anne Lee's Shakers, Joseph Smith's Mormons, and John Humphrey Noyes's Oneida Community.

Kern's introduction and first three chapters succinctly present his psychosociological framework, his analysis of the problem of the self in Victorian culture, the cultural anxieties and ambiguities about sexuality and the "true" woman, and the Victorians' fears and fantasies about the communards' sex roles and sex life. Some two hundred pages are then devoted to the capsule biographies of the three charismatic leaders and a detailed presentation of sexual life in the utopian societies. Kern's final two chapters place these communes within the broader cultural patterns of Victorian America.

Kern believes that these utopian leaders' personal insecurities made them sensitive barometers of the culture's anxieties over sexual roles and practices. In particular, each leader had a special fear and distrust of the sexual, competitive woman. These fears, given ideological expression, united leader and followers in a community where an ordered love prevailed. Kern believes that each community, for profound psychological reasons, challenged the traditional belief in woman's moral superiority. In fact, sex and role relationships were institutionalized to assure morally superior men their dominance over women. Thus, these communities were far from the "abortive liberal reformism" (p. 5) that many historians have claimed.

Kern's most substantial contributions come in his chapters on the Shakers and Mormons where his use of demographic data, diaries and letters, and his own shrewd insights indicate how sexual concerns were an integral part of the utopians' "ethical, theological, and ideological structure" (p. 6). Other aspects of Kern's reasoning and argument are not as persuasive because his analyses of character and ideological content rest on psychological assumptions not adequately explained in the text or in his copious notes section. The brief psychobiographies of the leaders, "grounded in Freudian psychological assumptions" (p. 15), tell us that Lee was "masochistic in her obsession with sex, and sadistic in her revelation of the sins of others" (p. 75). Smith had paranoid traits, while Noyes suffered from a "neurotic almost paranoiac fear of betrayal" (p. 213). Historians insensitive to psychohistory will probably be annoyed by such statements.

Despite his awareness to the contrary, Kern's emphasis on the leaders' unconscious fantasies, as embodied in their ideology, often reduces the communities to mirror reflections of the leaders' disturbed

psyches. Kern's methodology is not sophisticated enough to account satisfactorily for the ties that bind leader and followers. Information about the intimate experiences of the communards is rare. Often, however, Kern asserts communal sexual practices from scanty evidence, especially on the Oneida community.

Even with these reservations, I think that this is a provocative book worth reading.

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JEAN STROUSE. *Alice James: A Biography*. Boston: Houghton Mifflin. 1980. Pp. xv, 367. \$15.00.

Jean Strouse's fascinating *Alice James* should be of interest to historians researching the family, women, medicine, or psychoanalysis, as well as a boon to James scholars and a joy to lovers of biography. The book begins before Alice James's 1848 birth with an important sketch of her father, Henry James, Sr., and proceeds to the childhood Alice shared with her four older brothers and their settlement (after peripatetic years in Europe and New England) in Cambridge, Massachusetts, when Alice was eighteen. Henceforward the story is divided by Alice's own breakdowns. Her first major one, climaxing an adolescence of "nerves" and "delicacy," inaugurated her career as an invalid at age 19; the next crushing one, in 1878 (when her eldest brother, William, announced his marriage to another Alice), confirmed it. From 1879—her career decided—through her death in 1892 we hear more of Alice's own point of view, most often in the trenchant, ironic commentary that earned her her reputation for brilliance. It is a measure of Strouse's accomplishment that the reader finds Alice James alternately highly admirable and entirely despicable, which is apparently how she saw herself.

Remaining caches of James family letters (with their peculiar felicities of language) have allowed Strouse to reconstruct the circuit board of family dynamics. The quixotic and contradictory, loving and controlling character of Henry James, Sr., dominates the book as he dominated his children's lives. Alice's different relationships with her brothers—particularly with William, who treated her as an erotic toy, and Henry, Jr., who sustained her with intense and sympathetic friendship—are compellingly drawn. Only Alice's relationship with her mother is slighted, a very regrettable omission, and one that must owe to lack of documents. Alice's prized friendships with women (and her antipathy to most men) agree with current historiography on "woman's sphere"; until her intimacy with Katharine Loring after 1879, however, she subordinated

all these to her family ties. The book might be subtitled "Daughter and Sister."

Strouse aims to allow Alice James to stand for herself, not for the average woman, or for the woman-as-victim, woman-as-undiscovered-heroine, or even the woman writer *manqué*. She succeeds. The biography will not boil down to simple rules for application to nineteenth-century society. Yet Alice's character does hint—only slightly and subtly, as is appropriate—of representativeness. Likewise, Strouse's sensitively nuanced, richly textured analysis of why Alice James was what she was suggests a more general reason why nineteenth-century women turned their talents to self-stifling: that is, because their sources of love and of constriction were the same—their closest male relatives.

The biography is perhaps most brilliant in its portrayal of the hothouse that was family life among the nineteenth-century New England elite—with its extraordinary but exotic blooms. The Jameses exaggerated the case by the father's as well as the mother's absorption in childrearing. Far from being a continuation of longer-standing tradition, that family life was a unique historical phenomenon with unique results for personal character, as the James children's lives illustrate.

NANCY F. COTT  
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KAREN J. BLAIR. *The Clubwoman as Feminist: True Womanhood Redefined, 1868–1914*. New York: Holmes and Meier. 1980. Pp. xv, 199. \$29.50.

Karen J. Blair has studied an important and neglected subject: the role of women's clubs in the popularization of feminist thought, attitudes, and self-perceptions. She argues that the experience of women in clubs was a significant factor in helping them recognize and assert their rights and responsibilities in public arenas. Focusing on the emergence within the club movement of "Domestic Feminism" ("the extension of women's domestically nurtured traits into the public sphere" [p. 117]), Blair asserts that because "Domestic Feminism" was more logically derivative of traditional conceptions of femininity than the less moderate feminism of early suffragists, the clubs were way stations to autonomy for large numbers of white, middle-class women.

Beginning with a brief discussion of the similarities between pre-Civil War moral reform societies and post-Civil War clubs, Blair lays out the progression of the club movement from the founding of Sorosis and the New England Women's Club in 1868 to the establishment of the General Federation of Women's Clubs in 1890 and the subsequent development of state and local federations. She provides evidence of the diversity of clubs and of their

growth in membership. The differences between Sorosis, which tended to include career women eager to overcome the pejorative attitudes and inequitable treatment they had encountered in the world of work, and literary clubs, which tended to draw older women bent on cultural education, are clearly drawn. Intriguing relationships between different clubs are also suggested, as in Blair's description of the (Boston) Women's Educational and Industrial Union, a cross-class organization that focused on working conditions and served as a model to encourage greater emphasis on civic reform within other initially more literary clubs. Finally, the changes over time in the goals and programs of the clubs are carefully noted.

Blair's book presents new information and offers an interpretation of the clubs that confirms and is confirmed by other recent studies of female associations. Unfortunately, however, the book lacks depth and context. It fails to explore the connections between the club movement and other contemporary developments in American history, and it does not set the experience of women in clubs within the context of their larger experience. Further, Blair's insistence that clubwomen were "feminists under the skin," whose overt acceptance of traditional roles provided "ideological cover" (p. 1) for their more authentic and feminist sentiments, prevents her from adequately exploring the subtle complexities and contradictions that were and are integral to feminist thought and consciousness. The book is a well-written reconstruction of the development of clubs, but it does not offer genuinely new insight into the changing status of women.

ELLEN CONDLIFFE LAGEMANN  
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CHARLES W. CHEAPE. *Moving the Masses: Urban Public Transit in New York, Boston, and Philadelphia, 1880–1912*. (Harvard Studies in Business History, number 31.) Cambridge: Harvard University Press, 1980. Pp. vii, 285. \$18.50.

Two themes in recent historical scholarship provide the analytic framework for this excellent study of the relationship between innovations in urban public transport and the development of regulatory policy. Historians of business are demonstrating that the modern structure of complex, high-volume, and technologically oriented firms rests on the innovations of professional managers. Simultaneously, urban historians are showing that mass transit administered by private firms functioned as an important facilitating mechanism for the physical growth and spatial reordering of cities. These complementary themes inform Charles W. Cheape's ac-

count of the development of electrified trolley and subway service in Boston, New York, and Philadelphia.

After 1880 millions of people arrived in these cities, at first concentrating themselves near downtown and then increasingly taking advantage of horse-drawn and mechanical transit to relocate along the urban periphery. So sharp were the increases in demand for transport that local businessmen rapidly expanded competitive omnibus lines and experimented with clumsy and expensive forms of motive power such as steam-drawn cables. Traffic increases and innovations in motive power quickly outpaced public policy, leaving urbanites dissatisfied with politicians and trolley operators.

Beginning in 1890 the electric streetcar and subway promised speedy and reliable service, but their immense construction costs demanded city-wide operations and, as a consequence, city-wide regulations. Local factors, according to Cheape, shaped the process of transport and policy innovations thereafter. In Boston, slower growth, congestion, and the topography of its downtown precluded costly experiments with cables, and cooperation between Irish and Yankee politicians encouraged the rapid development of public agencies capable of directing subway construction. In 1898 Boston was the first city to complete its subway, and it did so under the direction of a public agency. By contrast, the excesses of machine politicians encouraged Philadelphians to rely on private initiatives to transit development. Not until 1907 did leading merchants prepare a plan to complete the subway, and not until 1913 did city officials create a staff capable of directing rapid transit facilities. In each city, professional managers were ultimately placed in charge of transit operations, allowing the rationalization of firms and systems.

Cheape's handling of technological innovation often leaves one dissatisfied. For example, his invocation of technological lag, a concept popularized during the 1930s by sociologist William F. Ogburn, is curiously mixed with the more recent idea of consumer pressures toward utility regulation during the Progressive era. Self and community interests, not technological lag, conditioned the timing, adoption, and regulation of mass transit systems, as Cheape himself largely demonstrates. Never did the intense competition in each city—say, between commuters located in the core and at the periphery, and among the directors of large and small businesses, property owners along the transit corridor, and reformers—produce a self-conscious consumer movement around the issue of mass transit. Urban culture, local politics, and entrepreneurship, among several factors, informed transit regulation during the Progressive decades, again a point made abundantly clear by Cheape.

This book amplifies, nonetheless, the emerging picture of urban, business, and technological America. Cheape provides the best account we have of the processes of transit construction and regulation. He substantially succeeds in showing, through comparison, that local circumstances, especially politics, introduced modifications in the operations of trolley firms, thus pointing to an environmental restraint on strategies and structures in technologically oriented firms. *Moving the Masses* stands near the edge of an important new synthesis in the history of technological and business innovation and the patterns of urban growth.

MARK H. ROSE

Michigan Technological University

GUNTHER BARTH. *City People: The Rise of Modern City Culture in Nineteenth-Century America*. New York: Oxford University Press. 1980. Pp. viii, 289. \$19.95.

Historians of the nineteenth-century American city have identified a dizzying array of change and upheaval: migration and immigration, with their attendant social disruption; racial and class conflict; economic strain from fluctuating business cycles and shifting modes of production; political confusion as different groups wrestled for power; a general breakdown of the sense of community; and more. Yet cities continued to grow, and urban life did not dissolve into chaos. How under the circumstances was some modicum of stability possible?

Gunther Barth offers some answers, and in doing so he opens a path down which few new urban historians have ventured. Barth posits the notion that new institutions of urban culture—the apartment house, the metropolitan press, the department store, the ball park, and the vaudeville theater—gave city people commonalities of experience and identification that enabled them to overcome their divisions. The apartment house eased the strains of crowding and recreated private space, which had been lost when single-family housing became too expensive and tenements became demeaning. The metropolitan press provided a new form of communication that adequately replaced gossip with human interest stories and informed people with advertisements. The department store gave women a stake in downtown and made shopping an egalitarian social activity. The ball park, where baseball was played, taught men that rules governed behavior even in a free society and enabled immigrants to overcome rejection by learning the subtleties of a uniquely American phenomenon. And vaudeville bonded people together with humor and music that touched the common chords of human hope and frailty.

*City People* is a blend of intuitive generalization



and focused analysis. It is speculative, provocative, sometimes repetitive, and always engaging. It provides enlightening cross-national perspectives, particularly regarding origins of the department store and vaudeville, and it includes a cluster of fascinating drawings and photographs. Perhaps Barth becomes a bit too celebrative in asserting the harmonious potential of the new urban culture so that he underrates the persisting presence of class, ethnic, and racial conflict. And perhaps he never quite clarifies the process whereby people shape institutions just as they are influenced by them—and thus the book may be mistitled because Barth makes institutions rather than people the acting force. Nevertheless, Barth has written a solid, convincing work that should catch the attention of social and cultural historians.

HOWARD P. CHUDACOFF  
*Brown University*

DANIEL NELSON. *Frederick W. Taylor and the Rise of Scientific Management*. Madison: University of Wisconsin Press. 1980. Pp. xii, 259. \$19.50.

The most indigenous and ecumenical of American faiths may well be the cult of efficiency. As the principal prophet and generally accepted father of "scientific management," Frederick W. Taylor has long fascinated popular writers and scholars alike. Daniel Nelson, however, believes this treatment has tended to be misleading. Taylor, in his view, has been largely depicted in terms of ideas and interests present only in his later work, the writings from 1910 to his death in 1915. Since Taylor's major activity at this time was as a publicist for "scientific management" in an era of reform, reliance on these years has exaggerated his role as an innovator in labor management while neglecting his more important earlier contributions to the technology and organization of the factory system.

To obtain a proper perspective on Taylor and his significance, Nelson sensibly asserts, one should consider the development of his thinking as reflected in the chronology of his work. According to this study, Taylor's career consisted of three distinct phases. In the first, 1880–1901, Taylor was an inventor and engineer who gradually extended his purview beyond the machine as he recognized that mechanical improvements required organizational changes in order to achieve important results. In stressing the need for experts and emphasizing systematic methods of analysis and operation, Taylor contributed greatly to what Nelson in an earlier work, *Managers and Workers*, has described as the origins of a new factory system. This differed from the first in replacing a practice of dispersed authority, depending chiefly on foremen employing a "rule of

thumb" approach, with centralized managerial authority. In this meticulous account of Taylor's early career, Nelson offers invaluable insight into Taylor's slow integration of his ideas and techniques into a system, while also demonstrating that the results were neither as successful nor as scientific as Taylor liked to claim.

By 1901, Taylor had arrived at his theory of "scientific management," and little change of consequence occurred after that. Now, independently wealthy as the result of royalties and shrewd investments, he devoted himself to acting as an advocate for a small group of consultants who sought to introduce his approach into all aspects of American industry. Until 1910, however, Taylor was little known to the public. Then as the author of the *Principles of Scientific Management* and as an ally of Progressive reformers in their criticism of "big business," he emerged as the leading spokesman for the engineer's approach to industrial change with its stress on efficiency and productivity as social nostrums.

"Scientific management" had little to say about employees. Taylor believed workers were interested only in wages, and he was disdainful of efforts to improve morale, an approach known as "welfare work." His efforts, to the contrary, had been concerned with monetary incentive and weeding out weaker workers. By 1910, Taylor recognized that the "labor problem" was a major interest, and he encouraged reformers to seize upon "scientific management" as an instance of how the interests of capital and labor could be reconciled.

David Nelson has provided us with what will be a standard work on Taylor and "scientific management." My only criticism is that while he indeed relates Taylor to other advocates of systematic management, he has allowed himself very little room for other themes or digressions. Taylor is treated almost exclusively in terms of his career experience. While there is an effort at referring to broader economic developments, the social and intellectual trends of Taylor's lifetime are largely ignored. Nelson's work, in suitable tribute to its subject, is streamlined and efficient.

STANLEY BUDER  
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BRUCE PALMER. *"Man over Money": The Southern Populist Critique of American Capitalism*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1980. Pp. xviii, 311. \$19.50.

Although historians profess to be weary of old arguments, they keep coming up. Gilded Age scholars will recognize *"Man over Money"* as the latest entry in

the continuing battle over the meaning of the Populist experience in the United States.

The modern debate began with Richard Hofstadter's *Age of Reform* in 1956 in which the Populists were depicted as nostalgic agrarians intent on restoring an edenic past and convinced that they were victims of an international financial conspiracy. Their simplistic views and near paranoia made them harbingers of American right-wing demagoguery. Hofstadter's critics, including Norman Pollack, C. Vann Woodward, Walter Nugent, and Lawrence Goodwyn, reversed the picture. Their Populists were rational, tolerant, forward-looking precursors of the modern political left, either in its liberal or radical versions. It is impossible to separate Bruce Palmer's intellectual history of Southern Populism from this dialogue since he addresses himself directly to most of the issues it has raised.

Palmer is clearly in the anti-Hofstadter tradition and closest, it seems, to the views of Goodwyn. Yet "*Man over Money*" is a divided book. Palmer likes the Southern Populists and defends them against their detractors. At times he bends over backwards to squeeze radical significance out of his Populist editors, politicians, and letter writers. Through phrases like "the rudimentary class politics of a black-poor coalition" (p. 141) he suggests that Populists were proto-Marxists. He makes much of an "anti-monopoly greenback" thread within the movement. Unlike free silver, he says, this was inherently radical. Yet on the whole, he cannot in good conscience bring himself to make the Populists into original thinkers who possessed a consistent and truly radical critique of American capitalism. In effect, this book is a triumph of scholarly honesty over ideology.

What Palmer really demonstrates is something like the following. Southern Populists were essentially small businessmen or aspired to that status. They felt left behind when the late-nineteenth-century material spoils were divided and sought means to improve their share. At times they endorsed government ownership of essential services, but they never lost faith in private property, the profit motive, and free markets. Their focus on monetary solutions grew out of a venerable Jeffersonian-Jacksonian tradition, but it was an evasion of the real issues of wealth and power concentration. Their grievances made them sensitive to the injustices done other groups in Gilded Age America, but generally their sympathies for wage earners and for blacks were severely limited by the fact that they were rural white Southerners.

Palmer will probably not endorse this description completely, yet I believe it represents a fair reading of his interesting and useful book.

IRWIN UNGER  
New York University

ELMO RICHARDSON. *BLM's Billion-Dollar Checkerboard: Managing the O and C Lands*. Santa Cruz, Calif.: Forest History Society. 1980. Pp. x, 200. \$9.50.

In 1916 title to some 2.3 million acres of land, located in western Oregon and covered for the most part with the best stand of Douglas fir in the world, "revested" to the United States. Originally granted to the Oregon and California Railroad, it was now reclaimed. The United States Department of the Interior was given the management of the "revested" lands and, through the Division of the General Land Office and its successor, the Bureau of Land Management, has managed these lands ever since.

Despite its subtitle, this book is not a history of the management of the revested lands. Rather it is the story, at times very well told, of the clashes between and among the leading personalities and policies of federal forest management, state forest management, both federal and state politics, and the lumber business. Dealing with subjects notorious for the donnybrooks they have engendered over the years both on the Potomac and on the Willamette, Elmo Richardson is surprisingly evenhanded, although his sympathy for the BLM over the Forest Service and his commitment to sustained-yield forestry practices do shine through the screen of objectivity.

Many subjects are slighted. The story of Oregon's and other land grant railroads is relegated to the prologue. The tax travail and subsequent wealth of the eighteen O and C counties is touched upon only as it affected the policy clashes being recorded. The technical aspects of forest management are virtually ignored, and but few of the statistical results of BLM policy are presented. There is no thorough analysis of the oft-repeated charge that BLM policy has favored the large landholders and large mill owners at the expense of the small operators. Finally, the alleged overcut in the Rogue Valley in the decade prior to 1976 is dismissed with a mention in the epilogue.

All of which is understandable. Richardson disclaims completeness at the outset, recognizing that his is a pioneer work opening the territory for others to explore more fully. For the trailblazing he is to be congratulated, as is the Forest History Society for sponsoring his work.

LESLIE E. DECKER  
Salem, Maine

JAMES A. WARD. *J. Edgar Thomson: Master of the Pennsylvania*. (Contributions in Economics and Economic History, Number 33.) Westport, Conn.: Greenwood Press. 1980. Pp. xviii, 265. \$25.00.

John Edgar Thomson (1808–74), who built and long dominated the Pennsylvania Railroad, is the subject of this volume. Thomson was the son of a Pennsylvania farmer who worked on the side as a civil engineer. The younger Thomson learned engineering from his father and from on-the-job training. Starting railroad building in Pennsylvania at an early age, he became so effective an engineer that he was selected in 1834 as the chief engineer to build the Georgia Railroad (Augusta to what became Atlanta). He built well. No bankruptcy has ever marred the company, and it is still profitable. After thirteen years in Georgia, Thomson was a man of mature experience in railroad building and development. In 1847 he accepted the job of chief engineer of the unbuilt Pennsylvania Railroad, and from 1852 to his death he was its president. The company became the largest corporate organization in the United States and a landmark of good planning, good management, and solid effectiveness.

Thomson, whose personality was colorless, differed markedly from Cornelius Vanderbilt, Jay Gould, and John W. Garrett, who represented rival interests of the Pennsylvania Railroad. Basically he was a builder, not a manipulator. He selected brash but able young men, notably Herman Haupt, Thomas Scott, Andrew Carnegie, and William Palmer, and delegated to them great authority. They, with various others, became an informal but close-knit body of Philadelphia associates, one of the most effective entrepreneurial groups in the United States between 1850 and the Panic of 1873. Thomson and his associates engaged in many private speculations and sought to form coast-to-coast railroad combinations, but Thomson was very cautious about committing the financial assets of his company. He became wealthy but never built a great fortune. No scandal ever touched his career. The dearth of extant sources relating directly to Thomson has discouraged previous biographers, and it made James A. Ward's task difficult. The book pieces together a credible account of the life of Thomson, but it touches only tangentially most of the important subjects and great projects of development in which Thomson and his associates were involved. Great efforts yet need to be applied to unravel and analyze the forces operating in that era.

JAMES F. DOSTER

*University of Alabama*

MARK THOMAS CONNELLY. *The Response to Prostitution in the Progressive Era*. Chapel Hill: University of North Carolina Press. 1980. Pp. x, 261. \$18.00.

This book of essays is focused upon the largely unexplained and relatively unexplored antiprostitution campaigns of the Progressive era. Mark

Thomas Connelly presents us with a "cultural" study of antiprostitution thought and action that reveals "contradictions, limitations and possibilities of the cultural context in which antiprostitution flourished." He alternates between uncovering shared assumptions among antiprostitution forces and defining symbolic meanings of the reform, usually as a defense of "Civilized Morality." Further, he seems to apply critical theory by holding antiprostitution ideologies against actualities and assumptions against realities. This method provides numerous insights, a number of which are novel.

On the whole, the essays on women, immigration, the white slavery scare, the Chicago Vice Commission, and antiprostitution in the First World War extend our knowledge of this significant movement. They do not, however, give us the symbolic meanings promised. One reason for this shortcoming lies in the undifferentiated treatment of antiprostitution thought. The historical actors are not related well to various movements that gave their thinking prominence. Another failure, probably related to the general state of scholarship in the field, is the absence of issues, originating with minority and dissenting groups during the Progressive era, and contributing to alternative meanings for "Civilized Morality." Moreover, the social hygiene movement—a major source of antiprostitution agitation—contained preventive reforms touching on child rearing, education, and the social institution of youth. Although mentioned occasionally in this study, they are not dealt with sufficiently to give a more balanced view of symbolic meanings.

Connelly correctly defines antiprostitution thought as a new jeremiad used to preserve "Civilized Morality." For Puritans, the jeremiad served to conserve the bonds of a threatened community. Progressives, however, used their jeremiads for new purposes—to touch the feelings of guilt and shame in order to extend "Civilized Morality" into a reconstructed community. To get at meanings of antiprostitution thought, a connection must be forged among jeremiads from other antvice reforms. Taken as a whole, they point to an effort to redefine moral boundaries of commerce and, through regulation of social life, an effort to raise the importance of human life over the antivalues of an unrestricted market place. Perhaps, as Connelly suggests, antiprostitution was motivated by sexual fears and anxieties, carried to excess, but it was also motivated by social visions as well.

The cooling of the crusading spirit in the 1920s did not lead to antiprostitution's "swan song." It did end as a social movement. Nevertheless, it also had an institutional legacy into the 1920s. Further, New Dealers did not draw up a code for Mrs. War-

ren's profession, as the author, apparently with some humor, suggests. Rather, the Public Health Service embarked on a new campaign against prostitution and venereal diseases during the Second New Deal.

Connelly's contribution joins a growing number of studies that point to a substantial transformation of sexual attitudes and practices during the Progressive era. This study is limited by the paucity of historical research and publication on the topic. Given the status of research on symbolic meanings of sex and the changing concept of human nature, we do not have, as yet, a definitive statement on anti-prostitution ideologies or their symbolic meanings.

DAVID PIVAR  
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Fullerton

MELVIN I. UROFSKY. *Louis D. Brandeis and the Progressive Tradition*. (Library of American Biography.) Boston: Little, Brown. 1981. Pp. vi, 183. \$11.95.

In order to fit his very considerable knowledge of Justice Brandeis into the confines imposed by the Library of American Biography, Melvin I. Urofsky has been forced to cut and summarize. He has wisely chosen to treat Brandeis's early years lightly, since these have recently been treated in Allon Gal's *Brandeis of Boston*, and to concentrate on Brandeis's roles as "people's attorney," as a major figure in American Zionism (about which Urofsky has also written), and as Supreme Court justice. Brandeis was a brilliant law student and Boston lawyer, "more Brahmin than the Brahmins," who became wealthy and entered the reform lists, like so many Progressives, not fighting for his own immediate self-interests but for his concept of a just society. Nationally known as a reform lawyer after he, with help from Josephine Goldmark and Florence Kelley, prepared the "Brandeis Brief" in *Muller v. Oregon* (1908), he also became, after 1914, and important figure in American Zionism.

Woodrow Wilson's nomination to the Supreme Court of his long-time advisor provoked a bitter fight in which, says Urofsky, Brandeis's Jewishness was only a small factor. On the court (1916-39) he wrote eloquent opinions, defending free speech and other civil rights. He continued his long-standing opposition to bigness in business, and he was a guardian against tilting the federal system too much in favor of the central government at the expense of the states.

Like many biographers (including this reviewer), Urofsky tends to see things through the eyes of his subject, so that this book is not the place to look for critical analysis. In the very last paragraph Urofsky says that Brandeis's economic precepts "flew in the

face of irresistible historic forces," but this possibility is hardly considered in the course of the book. The "Bull Moosers" are misguided in 1912, Wilson is "naive" until Brandeis educated him, the New Deal starts down the wrong road until in the "Second New Deal" Roosevelt adopts Brandeis's ideas. Urofsky even quotes Roosevelt as saying "Brandeis is one thousand percent right in principle" (p. 164). According to Arthur M. Schlesinger, Jr., in *The Politics of Upheaval* (p. 651), that was only the first half of a sentence whose second half began with "but" ("but in certain fields there must be a guiding or restraining hand of Government because of the very nature of the specific field"). One might also ask whether Brandeis's nearly universal opposition to bigness, whether in business or law schools, was in fact rigidly ideological and not in keeping with his own views of a "living law"?

Brandeis's Zionism is also not really examined. Urofsky says that one of Brandeis's basic principles was not only to know the law but to know the facts. In the case of Zionism he seems to have known next to nothing about the Ottoman empire, the Arab world, or indeed anything else about the area he favored for a Jewish homeland. Like many Zionists, Israel was for Brandeis a mythical Shangri-la, without physical, political, or social reality.

Urofsky does well the difficult task of summarizing the most significant of Brandeis's Supreme Court decisions. As an introduction to the man and his ideas, one could hardly do better than this brief work.

DANIEL LEVINE  
Bowdoin College

PETER C. ENGLISH. *Shock, Physiological Surgery, and George Washington Crile: Medical Innovation in the Progressive Era*. (Contributions in Medical History, number 5.) Westport, Conn.: Greenwood Press. 1980. Pp. xi, 271. \$25.00.

Like so many areas in the history of American medicine, surgery has suffered from scholarly neglect. Even those landmarks of the nineteenth century, anesthesia and Listerism, remain largely unexplored, and virtually nothing is known about the practice of the average surgeon. Thus it is a pleasure to welcome the publication of Peter C. English's revised dissertation on the history of shock, "the leading cause of surgical death" in the late nineteenth century (p. 20).

English's study centers on the career of George Washington Crile (1864-1943), a Cleveland surgeon who fought a life-long battle to understand the nature of shock and to eliminate it from the operating room. After introductory chapters tracing the history of ideas about shock before Crile and the



growth of radical surgery in the late nineteenth century, English turns his attention to Crile, discussing in turn his medical training, his early industrial practice, his pioneering experiments on dogs in the mid-1890s, his identification of lowered blood pressure as the primary cause of shock, his influential suggestions for reducing the incidence of shock during surgery, his quarrels with professional physiologists, and his mobilization of surgeons during the First World War. Although Crile's views gained widespread acceptance during the early twentieth century, by the late 1910s most scientists had abandoned his theory of lowered blood pressure in favor of a model that explained shock in terms of a reduced volume of circulating blood.

From this informative account we learn not only about an important episode in the history of surgery but also about the complex relationship between science and medicine at the turn of the century. Nevertheless, this book will appeal more to specialists in the history of medicine than to general historians. Although English attempts to link Crile's "progressive medicine" with the Progressive movement of the early twentieth century, his efforts to place his subject in a broad cultural context are superficial. He asserts, but makes no effort to demonstrate, that Crile was "representative of many surgeons in his generation" (p. x). He credits Crile with fathering a "therapeutic revolution" that ostensibly saved many lives but fails to assess the practical effects of this revolution in terms of reduced mortality. It is also a shame that someone did not devote greater care to editing and proofreading this book.

RONALD L. NUMBERS  
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Madison

STEVEN A. RIESS. *Touching Base: Professional Baseball and American Culture in the Progressive Era*. (Contributions in American Studies, number 48.) Westport, Conn.: Greenwood Press, 1980. Pp. xiv, 268. \$22.95.

Rather like Babe Ruth, who is said to have expressed astonishment when told he was to receive money to play ball, Steven A. Riess seems dazzled at his opportunity to write a monograph about his favorite game. Riess, like Ruth, is a professional, and *Touching Base* is an excellent contribution to social history. The book has a brightness of tone that shows through the author's scholarly overlay of statistics, maps, tables, and commentaries on American myths, Progressive realities, land values, and rates of social mobility.

The central but by no means only thesis is that baseball functioned as a social integrator at a time when the processes of modernization, disturbing enough in any event, were made even more disruptive by the influx of millions of Catholic and

Jewish immigrants ignorant of the "real" America. To its publicists, baseball seemed to be a school of rural virtues, a creator of urban community, and the very model of social democracy. Riess documents the widespread belief in these characteristics and shows through careful analysis that there was a discrepancy between the myth and the reality. Baseball was born in the city and flourished in urban environments. Despite pastoral rhetoric and homilies about the old virtues, baseball owners often worked hand in glove with political machines, whose influence was useful when a building code was violated or when improvements in mass transportation could make a franchise more profitable. The bosses were, for their part, eager to be seen as good sports, and many of them doubtless believed that baseball was a splendid game and one that ought to be available to their constituents (even on Sunday, which meant a long battle with the predominantly W.A.S.P. sabbatarians). A less happy aspect of the partnership between baseball magnates and urban politicians was that professional baseball became "a nexus between politics and organized crime" (p. 61). Although baseball did function to some degree as a social integrator, Riess shows how owners tended to make the game available mainly to middle-class spectators by raising ticket prices beyond what most working men could afford, by scheduling games at the hours and on the days most convenient for the white-collar and professional groups, by building their new stadiums in "good" neighborhoods, and by luring fashionable females to the ball park with Ladies' Days, better seats, and reassurances about the game's respectability. Although the nineteenth-century players were often Irish- or German-Americans, there was a marked tendency in the Progressive era for the players to be better educated than the average American and to come from middle-class rather than from working-class homes. (The players' fathers were three times as likely to come from professional homes as the average male of 1910.) Despite the discrepancy between journalistic rhetoric about democracy and the reality of skewed advantages, baseball did serve, especially in the 1920s, as an assimilator of the immigrant and as an integrative social myth. One of the great virtues of *Touching Base* is that its author is a definer and discriminator, not simply a debunker.

ALLEN GUTTMANN  
Amherst College

STEPHEN VAUGHN. *Holding Fast the Inner Lines: Democracy, Nationalism, and the Committee on Public Information*. (Supplementary Volume to the Papers of Woodrow Wilson.) Chapel Hill: University of North Carolina Press, 1980. Pp. xiv, 397. \$21.00.

"I am Public Opinion!" declared one of the Liberty Loan posters issued by the U.S. Committee on Public Information during World War I. "As I judge, all men stand or fall!" Mindful of the power thus wielded by opinion, Woodrow Wilson and his circle did all they could to shape and mold it. When necessary—and often when hardly necessary—the Wilson administration set strict limits to wartime free speech. When so minded, moreover, the Wilson government pursued the dissenter, the critic, and the protester with a fury that had little precedent in American history since the Civil War—at least in the sense that federal power was officially mobilized in a repressionist cause.

In this book, Stephen Vaughn revisits the history of the CPI, the propaganda arm of the Wilson administration's efforts on the home front, a subject first opened up by James R. Mock and Cedric Larson in a far-ranging but fairly superficial work, *Words that Won the War* (1939). The context of the CPI is best learned from another work, *Opponents of War, 1917–1918* (1957) by Gilbert C. Fite and H. C. Peterson. Like briefer studies of the Wilson administration and civil liberties by Paul Murphy (1980) and this reviewer (1960), *Opponents of War* stresses the involvement of CPI and its leadership in official censorship and—more important—views the activities of CPI in the perspective of the Wilson administration's insistence on 100 percent loyalty from a citizenry that at the outset of war was badly divided on the issue of intervention. From this perspective, the only one that makes any sense to this reviewer, CPI's pamphlets, posters, little histories-written-to-order, and hortatory literature generally are part of a single, fairly coherent effort by the national government that merged vicious treatment of organized labor's left wing, suppression of minorities, harassment and imprisonment of dissenters, blatant neglect of the lynching problem, and an unrelenting chauvinism. That all this ultimately undermined Wilson's high-minded objectives *after* the war cannot revise the facts of the wartime situation.

Most of the foregoing is quite largely blocked out of Vaughn's version of why the CPI is significant. He contends that it is important to recognize some validity in the view of Harvey O'Higgins, associate chairman of the CPI, that the right of free speech had to be subordinated in emergency times. Correctly, Vaughn points out that CPI, in both its propaganda and its censorship functions, won approval of Clarence Darrow and Edward S. Corwin, among others. (Vaughn fails, however, to explore how many notables, for example, John Lord O'Brian, later came to repudiate their wartime positions on the matter.)

Exhaustive research in the CPI archives and many other manuscript collections has permitted Vaughn to reconstruct the role of CPI as a "nationalizing agent," on the one hand trumpeting the vir-

tues of nationalism (and chauvinism), on the other expressing a variety of views on the peculiarly "democratic" variant of nationalism that America—and its war effort—allegedly represented. Concentrating exclusively on domestic propaganda activities, the book draws from the myriad pamphlets and other materials of CPI to delineate how the agency's spokespersons justified intervention, pushed for conformity, "advertised America," and shaped the news. An enormous array of names passes by, chronicling the roles of intellectuals, hype artists, scholars, and dilettantes in the varied activities of CPI.

Although Vaughn apparently has screened at least a few CPI films, his analysis of this important aspect of the agency's work lacks close attention to film content. A brace of illustrations gives more substance to Vaughn's section on posters and advertising, and throughout the work he has made meticulous use of intra-agency correspondence and public statements to etch the views of Chairman George Creel and many other CPI officials and writers. What was actually happening out in the country—the official campaign against and destruction of the IWW, the violence and vigilante activity, the crushing of free thought in schools and universities, the imprisonment of Eugene Debs and some two thousand others under the Sedition Act and Espionage Act—is largely subordinated and kept out of sight. For this reason, though the book serves well its stated function of chronicling the CPI and probing the ideas of its staff of intellectuals, advertising people, and others, it comes terribly close to uncritical "court history."

HARRY N. SCHEIBER  
University of California,  
Berkeley

GEOFFREY J. MARTIN. *The Life and Thought of Isaiah Bowman*. Hamden, Conn.: Shoe String Press. 1980. Pp. xvi, 272. \$22.50.

The publication of *The Life and Thought of Isaiah Bowman*, the last in a triad of books that clarify the evolution of American geography through studies of three of its leading figures, confirms Geoffrey J. Martin's position as the leading practitioner of this form of scholarship in geography today. As biographies, these books are limited because Martin pays little attention to the personal lives of Ellsworth Huntington, Mark Jefferson, and Bowman, but as intellectual histories of a discipline and a time they are effective and revealing.

Bowman emerges from these pages as a man with a relentless drive and an extraordinary range of abilities. Raised in frontier conditions in rural Michigan in the late nineteenth century, Bowman applied his energy, self-discipline, and talents to be-

come a leading authority in geography, director of the American Geographical Society, president of Johns Hopkins University, and advisor to presidents.

Bowman's main creative contributions to geography were made before 1933. The major influences on his geographic thinking were Mark Jefferson and William Morris Davis. From Jefferson he developed an interest in South America, and throughout his geographic career he believed in the importance of regional synthesis. Davis's seminars at Harvard inspired Bowman, as they did so many geographers of this period, and led to his interest in physiography and "ontography" (the human response to the physical environment). Bowman was equally at home writing geomorphology, regional description, and human geography. His most significant works, *The New World*, written in 1921 in the wake of the Paris Peace Conference, and *The Pioneer Fringe* (1931) were prototypes for the geographic study of politics and frontier settlement respectively.

After 1933 Bowman became increasingly involved in administration and political advising. Under his directorship the American Geographical Society had developed into an important center for research, and its resources were used in the preparation of the American delegation to the Paris Peace Conference, in which Bowman played an important role. In 1935 Bowman became president of Johns Hopkins, a position he held until shortly before his death in 1950. He was an efficient administrator but not a popular one, particularly with the liberal professors who bore the brunt of his growing conservatism. His wide geographic knowledge, his acceptable political views, and his ambition made him a confidant of statesmen, and he participated in the Stettinius Mission (1944), the Dumbarton Oaks Conference (1944), and the San Francisco Conference (1945). Probably no other geographer has been as influential on political policy making.

Martin has written a thorough and elucidating account of an interesting man. It is perhaps a pity that he observed the demarcation line between "academic" biography and "true" biography so assiduously, because a deeper investigation of Bowman as a man would surely have shed more light on Bowman as a scholar, administrator, and diplomat.

DAVID WISHART  
University of Nebraska

LARY MAY. *Screening Out the Past: The Birth of Mass Culture and the Motion Picture Industry*. New York: Oxford University Press. 1980. Pp. xv, 304. \$19.95.

Research on the American motion picture industry has greatly expanded in recent years, but few film scholars have attempted to interrelate their specialized studies with the major themes of American historiography. This integration is the goal of Lary

May's book: he sees the rise of the motion picture industry as a central force in the transformation of American culture between 1890 and 1930 from Victorian values to modern mass cultural norms. Basic to his argument is the thesis that before World War I the film industry was "the handmaiden of Progressivism" (p. 66), that motion picture content was shaped by elite reform groups working through the National Board of Review to uphold traditional values of work and family in the new popular medium. With the decline of Progressivism, Douglas Fairbanks and Mary Pickford emerge as transitional cultural archetypes, embodying wholesome youthful energy, a new spirit within the framework of old ascetic norms. Finally, in the 1920s, the films of Cecil B. DeMille, among others, exemplify the full emergence of a mass cultural construct emphasizing leisure, play, sexuality, and consumption.

This is an interpretation of early film history far more promising in conception than execution. One essential weakness is the absence of evidence to support the claimed connection between prewar films and Progressivism. Although the more than four hundred films D. W. Griffith made for Biograph between 1908 and 1913 are critical to this argument, only three are mentioned, briefly, and the primary printed source, *Biograph Bulletins, 1908-1912* (1973), edited by Eileen Bowser, has not been used. Nor is evidence cited to demonstrate the power of the National Board of Review, which other scholars have considered largely ineffectual. Even more damaging, however, are the pervasive factual errors: names are misspelled, dates for films are incorrectly given, films are egregiously misdescribed, footnotes contain inaccuracies. Most seriously, the basic chronology of events in the history of the motion picture industry is altered so that the thesis may be buttressed: this permits the author to claim that traditional social groups held power over the motion pictures long after, in fact, Jewish immigrant businessmen had gained control in the industry. Film specialists have sometimes been castigated, and rightly, for failing to meet the standards of professional historians; this is a work by a professional historian that meets the standards of neither specialty. It is almost scandalous that a scholarly press should permit so many errors to go uncorrected into print.

ROBERT SKLAR  
New York University

CHARLES C. ALEXANDER. *Here the Country Lies: Nationalism and the Arts in Twentieth-Century America*. Bloomington: Indiana University Press. 1980. Pp. xiv, 336. \$32.50.

This is an ambitious attempt to survey the creative arts in the United States since 1900, both in their relationships to each other and to the larger society.

Charles C. Alexander has done a good job of digesting the literature and covering major national developments. His focus is on the intellectual intent and reception of the works involved rather than on technical analysis. The book's central theme is the tension between advocates of nationalistic art and those of some kind of cosmopolitan ideal. The narrative proceeds from delineating the genteel tradition that seemed dominant about 1900, to the attack made on it in the name of individualism and variety, to the triumph of creative arts centered on the artist rather than the audience. Alexander examines the energy in all these movements and their cultural implications for democracy and national ideals. He also reminds us that until mid-century the triumph of the "modern" or artist-centered ideal did not seem inevitable. He is also alive to the nuances and ironies in categories of thought such as traditionalism, which allowed for American exceptionalism even while seeking cosmopolitanism.

Alexander corrects many misconceptions that still appear in general accounts. He notes, for instance, that Louis Sullivan did not advocate mere functionalism but was a romantic nationalist, eager to develop forms that reflected the variety and energy of American life, coupled with a strong sense of tradition. By the same token, the Armory Show of 1913 did not burst on the intellectual community like a thunderclap. To Alexander, the 1920s was not a cynical or escapist period in intellectual matters. Like many recent scholars, he suggests that we see it thus because of undue focus on expatriates and untypical figures. He also sees the 1930s as a final period of hope for a national cultural style, as the Great Depression helped make Americans conscious of their shared past and alleged uniqueness.

The discussion of literary matters is familiar and that of painting breaks no new ground. Alexander's treatment of a variety of musical forms is insightful and will be fresh to nonspecialists, as is the case with his coverage of drama and dance. The many vignettes of leading protagonists, such as Van Wyck Brooks, H. L. Mencken, and Grant Wood, are generally shrewd and well done. Although the book is often a carefully phrased catalogue of events, it is well written and displays a refreshing amount of common sense and freedom from jargon. It should be valuable to nonspecialists, teachers, and students. It is thus a shame that Indiana University Press has priced it beyond the reach of most individuals.

H. WAYNE MORGAN  
*University of Oklahoma*

GEORGE M. MARSDEN. *Fundamentalism and American Culture: The Shaping of Twentieth-Century Evangelicalism, 1870-1925*. New York: Oxford University Press. 1980. Pp. xiv, 306. \$19.95.

George M. Marsden's book serves as the capstone of more than a decade of work by scholars on the thorny topic of the place of the fundamentalist movement in early-twentieth-century American religious and social history. Influenced by the antics of the Scopes trial and by the critical observations at that time of writers like H. L. Mencken, historians initially largely dismissed the fundamentalists as representatives of a small-town, rural, anti-intellectual perspective that was rapidly losing force in American life. In more recent years, however, writers like Ernest Sandeen, LeRoy Moore, C. Allyn Russell, and Marsden himself in preliminary sketches of the present work, have begun to create a more complicated picture that reflects more precisely the religious-theological and sociocultural milieu in which fundamentalism developed and flourished. The volume under review builds on these earlier studies yet goes beyond them all in its attempt to establish the full historical significance of fundamentalism.

Marsden's starting point is the assumption that the fundamentalist movement evolved out of nineteenth-century evangelical Protestantism, more specifically the many conservative manifestations of that movement in the late nineteenth and early twentieth centuries. This is an extremely broad contextual framework, yet Marsden ranges widely therein with great confidence and effectiveness. For example, the author comments extensively on such matters as the conservative revival leadership of D. L. Moody, the doctrinal pronouncements of theologians like Benjamin Warfield, Charles Hodge, and J. Gresham Machen, the rapidly developing holiness and millennial dispensationalist movements, and the contributions each made to the slowly developing fundamentalist movement. Marsden establishes clearly, and perhaps most significantly, linkages between the philosophical and theological assumptions of Scottish Common Sense Realism and Baconian philosophy that underlay much of nineteenth-century evangelical Protestant thought and the religious attitudes animating many who eventually joined the fundamentalists.

Yet Marsden also argues, persuasively for this reader, that fundamentalism did not become a fully self-conscious movement until during and after World War I. The impact of the war produced a cultural crisis in American life generally, which crystallized in adherents of fundamentalism a deep sense of alienation and frustration. These attitudes translated concretely into a political program against the teachings of evolution in the schools and in strident clashes with theological liberals. Thus Marsden effectively combines analysis of secular social forces with a sophisticated understanding of a complex theological and religious setting to achieve a new level of understanding concerning a major



American religious movement that continues in many ways to influence American life.

This work points once again to the flourishing state of scholarly studies of conservative American Protestantism in the past century. The author uses well the large secondary historical literature that has developed around this subject over the past decade or so, even incorporating information from a number of very useful dissertations and theses. In the concluding sections of the book Marsden also offers thoughtful speculations about the larger cultural and historical significance of fundamentalism, thoughts that are enhanced by fruitful comparisons with similar developments in England. In short, this is a superb monograph, to be recommended to all students of early-twentieth-century American culture and religion.

JAMES FINDLAY  
*University of Rhode Island*

EDWARD BERKOWITZ and KIM MCQUAID. *Creating the Welfare State: The Political Economy of Twentieth-Century Reform*. (Praeger Special Studies.) New York: Praeger. 1980. Pp. xv, 185. \$22.95.

Most historians come to the study of social welfare by researching social workers, volunteers, reformers, radicals, unions, religious leaders, voters, and parties. Edward Berkowitz and Kim McQuaid heavily emphasize corporation "welfare capitalists" in juxtaposition with "welfare bureaucrats." Readers of Clarke Chambers, Roy Lubove, and James Leiby may find the resulting monolithic thesis useful yet strangely disturbing.

The authors conclude that there was "a close, even symbiotic, relationship between private businessmen and public bureaucrats that resulted in the creation of an American social welfare system" (p. 164). Conventional wisdom is myth because these groups like to portray themselves as mutually independent "when in fact they are not today and have never been so in the past." Businessmen brought bureaucrats into the welfare system; "laissez faire, applied to large corporations, was always a myth." Welfare bureaucrats owe their very existence to businessmen who led the way in the Progressive era toward government welfare programs, so that minimum wage laws and workmen's compensation emerged (with some trade union collaboration) at the state level. Social Security Act old-age pensions allegedly were "consciously based" on American life insurance practices. Unemployment and disability insurance were established with sturdy employer support in order to solve their own problems. Again, "Between 1900 and 1950 private businessmen and public bureaucrats operating on both the state and federal levels created an American social welfare system" (p. 164).

Yet Aid to Dependent Children is not indexed nor are Norman Thomas, Socialist Party, Robert M. LaFollette, Abraham Epstein, WPA, Supreme Court, Hubert Humphrey, Michael Harrington, and so forth. The book stops at 1956, so that President Lyndon Johnson, OEO, War on Poverty, Medicaid, and Medicare are missing. Research was heavy in the manuscripts and reminiscences of corporation figures and Department of Commerce archives, and the text, notes, and bibliographical essay do reveal serious efforts at analytical thought.

The authors contribute to our knowledge of the development of workmen's compensation and unemployment insurance and the thoughts and conduct of corporation leadership. But a book so quiet on familiar themes (progressive and liberal leaders and voters as expanders of the national government, social workers as agitators, the arousal of the national conscience, and public opinion and religious humanitarianism as causation for change in a democracy) cannot be said to justify so comprehensive a title.

VAUGHN DAVIS BORNET  
*Southern Oregon State College*

JACOB FISHER. *The Response of Social Work to the Depression*. Boston: G. K. Hall. 1980. Pp. xxi, 266. \$23.95.

During the 1930s, the United States made the transition from no federal involvement in welfare programs to a permanent, federal welfare structure. Jacob Fisher explores how the social work profession both sought to influence and was influenced by this transition.

Fisher's background is a part of this book. During the Great Depression, Fisher, who has a master's degree in social work, worked for private social service agencies while actively participating in social work's rank-and-file movement. He helped edit *Social Work Today*, the mouthpiece of the profession's radical wing, and he was active in the early efforts to unionize social workers. Fisher's personal acquaintance with some of the people about whom he writes, such as Mary Van Kleeck, makes for several vivid portraits. One wishes he had included more anecdotes and impressions.

Social work did not have a single, unified response to the New Deal. Fisher describes social workers supporting the New Deal, some becoming New Deal administrators, and others being critical of the New Deal for not moving farther to the left. His emphasis is on the radicals as represented by the rank-and-file movement. These were social workers interested in unionizing their profession as well as in raising the political consciousness of their colleagues and forming them into pressure groups for better welfare programs.

The weaknesses of this book may stem from the

fact that Fisher is not a professional historian. He provides plenty of general background on the 1930s but mentions groups like the Unemployed Councils and the Workers Alliance without ever stopping to explain them or even to identify them as Communist, Socialist, or an uneasy coalition of the two. Programs such as public housing and food stamps (first tried in 1939) are ignored as are the contributions of settlement-house workers to improving welfare. Fisher uses only about a dozen general books on the 1930s, not mentioning a number of more specific, related works. Except for very limited use of the Social Welfare History Archives, he has made no use of archival sources. His footnotes citing journal articles list only the journal title and date—no author, no article title, and no page reference. These are drawbacks that will limit this study's usefulness to scholars.

Fisher's chief contribution is that he provides information on social workers' attitudes, particularly those of the rank-and-file movement, not easily obtainable elsewhere.

JUDITH ANN TROLANDER  
University of Minnesota,  
Duluth

DONALD A. RITCHIE. *James M. Landis: Dean of the Regulators*. Cambridge: Harvard University Press. 1980. Pp. ix, 267. \$15.00.

James M. Landis played many roles in his public life: draftsman of the Securities and Exchange Act, federal regulator under FDR and Truman, dean of the Harvard Law School, and finally a Kennedy family lawyer. Certainly Landis in his public life was a "legend" in his own day; yet there was another side to Landis, a tragic personal life. In portraying these two sides of Landis, Donald A. Ritchie masterfully integrates business history and biography into a study that is thoroughly researched, well written, and at times, moving.

Landis developed, early in life, an insatiable desire for recognition and success. A protégé of Felix Frankfurter and a law clerk to Justice Louis Brandeis, the twenty-eight-year-old Landis became a professor at the Harvard Law School in 1928. He was called by the Roosevelt administration to draft, with Benjamin V. Cohen, the Securities Act of 1933 and the Securities and Exchange Act of 1934. Many businessmen opposed Landis's subsequent appointment to the Securities and Exchange Commission, but he soon won the confidence of the business community. In the process Landis developed a friendship with the SEC's first chairman, Joseph P. Kennedy, whom he replaced as chairman in 1935.

In many respects Landis's service on the SEC was the high point of his public career. In late 1937 he left the SEC to become the youngest dean in the

history of the Harvard Law School. With the outbreak of war Landis headed the Office of Civilian Defense before Roosevelt selected him to represent American interests in the Middle East as Director of Economic Operations. His failure to convince Roosevelt that American interests lay in an alliance with anti-Zionist Arab states, and not in the creation of a Jewish state, gave evidence of Landis's minimal influence in the Roosevelt administration.

By 1944, Landis's behavior had become increasingly erratic. Following his return to Harvard in 1945, he divorced his wife in order to marry his secretary. He escaped the ostracism of Cambridge society only by accepting Truman's offer to join the Civil Aeronautics Board. Eighteen months later, Truman asked for his resignation. Landis's drinking, common gossip in Washington, provided ammunition for his enemies, particularly Pan American Airlines, whose plans to monopolize overseas air travel he had thwarted.

For the next decade Landis worked for Joseph Kennedy and family, undertaking legal work, helping Robert Kennedy write the Army-McCarthy hearings report, and doing much of the research for *Profiles in Courage*. With JFK's election in 1960, Landis undertook, at the request of the transition team, a *Report on the Regulatory Agencies to the President-Elect* (1960). During the New Deal, Landis had eloquently celebrated the independent regulatory commission; now he urged greater presidential control over the commissions. Landis's return to Washington politics ended abruptly when the Internal Revenue Service discovered that he had evaded paying income taxes since 1956. Trial, imprisonment, and disbarment brought disgrace to Landis. Less than a year later he died, a possible suicide.

Ritchie is at his best in tracing the final decline of Landis. His sensitive portrayal of Landis is based on extensive research in the Landis papers, the Roosevelt and Truman libraries, and the Harvard Business School, as well as on interviews with key participants, including Landis's psychiatrist. Ritchie might have generalized more about the nature of regulation in capitalist America. Described by Joseph Schumpeter as "capitalism in an oxygen tent," the economy, which Landis helped design, emerged in the postwar period moderately reformed, incoherently regulated, and dependent on government. Nevertheless, Ritchie has written a fine biography of a man driven by ambition and a desire to serve the public interest.

DONALD T. CRITCHLOW  
University of Dayton

WILLIAM GRAEBNER. *A History of Retirement: The Meaning and Function of an American Institution, 1885-1978*. New Haven: Yale University Press. 1980. Pp. x, 293. \$22.50.

As William Graebner candidly admits at the outset, his is "something less, and something more, than a summary of the history of retirement in America" (p. ix). What makes it more is his attempt to stand the reformist account of the social invention of retirement on its head—or, more precisely, on its economic bases. The creation of a pension-supported, workless stage of life, he argues, has never had much to do with the welfare of the elderly. Private pension and retirement plans were born out of the needs of corporate capitalism for a means of rationalizing, depersonalizing, and therefore making possible the painful act of firing superannuated employees. Labor unions took up the idea in part in reaction to the intensified work pace and in part in an effort to reduce unemployment by reducing the number of job seekers. New Dealers, he contends, seized on public old-age pensions essentially because they saw them as a relief measure capable of shifting the incidence of unemployment away from younger, more volatile, and somehow more important workers. If in the 1970s the social security—buttressed policy of forced retirement of the old fell into disarray, it is not, Graebner argues, because the welfare of the elderly has finally gained the upper hand over economic imperatives but because international economic competition has once more mandated employment of the elderly.

Despite all these provocative invitations to reconsideration, most readers will be more strongly impressed with how much less this history of retirement delivers than its title and arguments promise. Graebner's section on the origins of the Social Security Act, for all its incompleteness, is the strongest part of the book. Elsewhere Graebner's evidence is considerably weaker and his choice of where to look for it puzzling. He rests his contention that elderly workers were seldom fired before the twentieth-century depersonalization of labor relations primarily on incidents carefully drawn from the National Archives, without apparently thinking to ask if the peculiarly paternalistic mores of federal employment extended to privately employed blue-collar workers—and, if they did, why workers did not think so. Other absences are equally conspicuous. Graebner eschews both the statistical and the social history of retirement. What the lot of an elderly worker was before the invention of old-age pensions is left for readers to guess, just as they are left to guess what the workplace battle over pension policies had to do with the intimately related battles over wage structures and seniority systems. Such things as dreams of leisure and the lobbying organizations of the elderly flit through his account with the furtiveness of epiphenomena.

What went wrong in a book built on so much hard archival work may ultimately have something to do with those archives themselves. Graebner

shaped his work, he reports, to such troves of material as seemed "capable of sustaining intensive analysis and rich description" (p. ix). Hence his minute attention to the Carnegie Foundation for the Advancement of Teaching and hence his disregard of Carnegie's steelworkers. But one would have thought that the lesson of the new social history was different than this: that sources do not make for historical richness as much as do sustained and rigorously pursued questions.

DANIEL T. RODGERS  
Princeton University

ELEANOR AMIGO and MARK NEUFFER. *Beyond the Adirondacks: The Story of St. Regis Paper Company*. (Contributions in Economics and Economic History, number 35.) Westport, Conn.: Greenwood Press. 1980. Pp. xi, 219. \$22.95.

This is a company-sponsored study written "to further the understanding of the role that St. Regis has played in using one of the most valuable natural resources" (p. x). The corporation wanted a history that would explain its development to historians, environmentalists, and those who work in the forest products industry. Eleanor Amigo and Mark Neuffer assert that they were allowed freedom of inquiry and interpretation as well as ready access to corporate records and employees.

The book does not present sophisticated financial analyses, nor does it relate St. Regis's growth to that of other natural resource corporations. The volume does contain, however, many interesting details on interrelationships among free market forces, technological innovation, politics, and natural resource development.

St. Regis was founded in upstate New York in 1899 after technical advances allowed wood pulp to be used for the manufacture of the cheap paper required by the new mass circulation dailies. The newsprint industry was highly competitive, however, and publishers with political influence furthered competition by getting Congress to allow Canadian newsprint into the United States duty free. Part of St. Regis's response to oversupply and low profit margins was hydroelectric power development. By the late 1920s almost half the company's assets were in electrical utilities, and its generating stations were powering variable-speed motors that allowed for a transition from newsprint to the production of the more profitable catalogue, directory, and magazine papers. St. Regis also began developing capacity for the manufacture of the "non-cultural" papers, especially multiwall bags and other materials made from kraft paper.

The company sold off its electrical holdings in the 1930s, but its strong position in kraft paper and

packaging allowed it to take advantage of the packaging revolution that swept America after World War II. As per capital consumption of paper products dramatically increased, St. Regis built new plants close to the sources of wood pulp in the South and Northwest. The corporation also was forced to spend large sums on pollution controls and meet the challenge of organized groups of environmentalists.

CHARLES F. CARROLL  
University of Lowell

WARREN ASHBY. *Frank Porter Graham: A Southern Liberal*. Winston-Salem, N.C.: John F. Blair. 1980. Pp. x, 386. \$20.00.

Frank Porter Graham became president of the University of North Carolina at Chapel Hill in 1930 and assumed the presidency of the newly created consolidated University of North Carolina in 1932, a position that he held until 1949. Dedicated to a vision articulated by educational reformers at the turn of the century, he saw the university as "the apex" of an educational system "nourished by and nourishing the life of the people of the state." The University of North Carolina continued to advance its leadership in Southern higher education under Graham who, as president, championed intellectual freedom and promoted the social welfare movement throughout the South. Although Graham was not a radical, his promotion of labor unions, civil rights for blacks, and the New Deal led many to dislike, and some to distrust him profoundly.

Franklin Roosevelt expressed his confidence in Graham by appointing him to the Advisory Council on Economic Security (chairman), the Advisory Committee to Study Vocational Education, and the Advisory Committee on Economic Conditions in the South (chairman). He served on the National Mediation Board (1941) and the War Labor Board (1942-45), commuting on the weekends to Chapel Hill to continue his duties as UNC president.

After the war, President Truman appointed Graham to the Committee on Civil Rights, which urged the end of segregation, although Graham saw education, not force, as the solution of the problem. In 1949, he was appointed to an interim term in the United States Senate but was defeated for a full term in 1950. In the campaign, he was vigorously attacked for his racial views and labeled as a "fellow traveler" because of his liberal affiliations, especially his service for twenty months as chairman of the Southern Conference for Human Welfare.

From 1951 to 1970, Graham served as the United Nations representative to India and Pakistan, but the intensity of the controversy there prevented the breakthrough he sought. He became one of the

most popular UN officials who did much to promote good will.

Warren Ashby's study does not attempt to analyze in depth the various movements in which Graham was involved, but it is an excellent biography that delineates Graham's weaknesses including his lack of attention to detail and his inability to counter racist arguments. Ashby does not allow his long-time friendship with Graham to affect judicious evaluations, and his portrayal of Graham will enhance the reputation he already enjoys.

HUGH C. BAILEY  
Valdosta State College

LISLE A. ROSE. *Assault on Eternity: Richard E. Byrd and the Exploration of Antarctica, 1946-47*. Annapolis: Naval Institute Press. 1980. Pp. x, 292. \$19.95.

Operation Highjump, which this book is about, marked the renewal of American exploration in Antarctica after World War II. It was a U.S. Navy project urged by Admiral Richard E. Byrd and accepted by the government primarily as a military move to test men, equipment, planes, and ships under polar conditions. It began in November 1946 and lasted into April 1947; thirteen ships and four thousand men participated, organized as three groups for air operations into the white continent.

The artistry and emphasis of this book lie in its stories of adventure and endurance. In one climactic chapter, Mariner George-I, a seaplane, crashed and burned, killing three of the crew and sparking a desperate search operation. In another chapter navy and coast guard skippers, pressed, banged, dodged, and churned their way through the ice. The setting for Lisle A. Rose's inspired prose is always the wind, the cold, the fog, the seas, the color, the vastness, and the silence of a continent, which he has personally experienced. In the literature of the antarctic there was room for a book of this genre.

Perhaps the devotion to narrative caused the author to skimp on relevant knowledge and analysis. He does not make a lasting theme of the natural rivalry between the group who had been with Byrd from 1928 on and the naval forces that henceforth were to be so prominent in the antarctic. Rather he portrays the rivalry mostly through one event—the crash of George-I and the subsequent reproaches by Byrd's followers. He communicates their opinion that the disaster was "initially avoidable" and afterward augmented by the "ignorance and inexcusable apathy" of the flight crew (p. 97). Yet he does not give enough information about the controversial flight, even as to its navigational resources, to enable the reader to understand fully the basis for the hostile statements or to make an independent judgment about the crash. Also, the author



should have constructed some figures of his own on the cost of Highjump, in the absence of statistics in the documents. Except for Paul Siple, the War Department adviser, the scientists in this book are shadowy figures whose plans and daily work are not detailed. Since some of them were contemporaries of Byrd, their behavior during the expedition might have helped in developing the theme of scientists and the military, so important in governmental projects.

Aerial photography was the most fruitful scientific activity, aided by a new system of multiple cameras. Mountain ranges, huge rivers of ice, and even one large place bare of ice and snow were noteworthy discoveries. Use of planes gave a chance for the magnetometer, which in measuring the intensity of the earth's magnetism identified the sedimentary or volcanic nature of the rocks under the ice and snow. Inevitably there were studies of ice movement, of snow and ice under pressure, and of the speed of sound through ice.

I would propose that the author do a biography of Admiral Byrd, but his papers are closed.

THOMAS G. MANNING  
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WILLIAM WHITNEY STUECK, JR. *The Road to Confrontation: American Policy Toward China and Korea, 1947-1950*. Chapel Hill: University of North Carolina Press. 1981. Pp. 326. Cloth \$20.00, paper \$10.00.

A scarcity of source material and the red herrings of conspiracy theories have shrouded the history of United States policy toward China and Korea after World War II. This important addition to the literature transcends the easy stereotypes about American efforts to cope with the Chinese revolution and the conflict in Korea. William Whitney Stueck, Jr., demonstrates that a complex interplay of Realpolitik, bureaucratic rivalry, personality conflict, and partisan domestic politics first restrained the U.S. from slipping more deeply into China's civil war and then propelled it headlong into a major conflict in Korea. He goes further than any predecessor in explaining how the recklessness in Korea evolved out of the restraint in China.

Stueck's evidence, from State Department, Department of Defense, and executive sources, should convince most readers that from late 1947 through early 1950 Secretaries of State George Marshall and Dean Acheson skillfully managed a limited withdrawal from the vortex of the Chinese revolution. Their leadership rested on the advice of both area specialists (John Carter Vincent, W. Walton Butterworth) and the "geo-politicians" (George Kennan and his policy planning staff—including John P. Davies who had a foot in both camps) who con-

cluded that the Nationalist regime was doomed and not worth saving. Their arguments, continually pressed upon a wary and cranky president, Congress, and military establishment, stressed that a Communist China would not really augment Soviet power and might even become a rival center of national communism. They spoke of a Sino-Soviet split and even sought to prepare the Congress for recognition of a Communist regime. Sadly, they were either unwilling or unable to do anything positive to improve contacts with the Communists. Instead, in an effort to purchase the "China Bloc's" support for European aid, the administration continued token support—military and economic—to the Nationalists. Little thought was given to how the Communists would interpret this policy.

Even as American policy makers choreographed their withdrawal from China, the implications of that retreat bore directly on Korea. Stueck demonstrates that the experience in China convinced State Department and other civilian officials of a need to vividly demonstrate U.S. "credibility" elsewhere in Asia. While China's "loss" could be reasonably explained as the triumph of a popular native revolutionary army against a corrupt and weak dictatorship, containment policy could not stand many such assaults. Drawing the line in Korea became, in a sense, the symbolic and therefore real test of U.S. credibility. Even though the generals dismissed the Korean peninsula as an irrelevant position, the diplomats saw it as a crucible for testing America's ability to sponsor and defend a containment network in Asia.

Despite this concern, the Truman administration failed to coordinate its signals regarding Korea. The Joint Chiefs of Staff, concerned with husbanding troops for a "total war," grumbled about any commitments to a strategic backwater. Chiang's irrepressible boosters in Congress sabotaged the Korean aid bill in late 1949 and early 1950 hoping to extort a new commitment to fight on the mainland or Taiwan. And even spokesmen for the State Department waffled in public when describing Washington's determination to stand by its Korean offspring.

These tensions grew larger during the six months preceding the outbreak of war. The Sino-Soviet pact undercut expectations for "Chinese Titoism." Taiwan continued to serve as base for Chiang, whose supporters in the Department of Defense became more assertive under the leadership of Louis Johnson. Dean Rusk's stewardship for Far Eastern Affairs in the State Department undercut Acheson's resolve to wash his hands of the Chinese civil war. Gradually, a consensus developed in favor of resisting any new Communist advance even in peripheral areas of Asia.

Stueck goes to some length in defending as well

as explaining Truman and Acheson's decision to fight in Korea. It was, the author argues, a real challenge by Moscow of the U.S. resolve to defend the postwar settlements. To make his case Stueck puts the best possible light on the South Korean regime and asserts that its popularity is demonstrated by the lack of success achieved by domestic uprisings or earlier infiltration. As originally conceived, American intervention in Korea was a limited operation, designed to restore a violated boundary and need not have poisoned American-Chinese relations. Stueck's explanation of these points, as well as his personality analyses of Marshall and General Douglas MacArthur, is spirited but not nearly so persuasive as the earlier material.

Not surprisingly, for a study of this scale, some important issues are obscured. Somehow the National Security Council and the policy papers evolved through it disappear. We learn little about how containment and credibility issues influenced the other trouble spots surrounding China—for example, Japan, French Indochina, and Indonesia. Some important policy initiatives and programs, such as the Mutual Defense Assistance Program that was the opening wedge in military aid to Indochina, are overlooked. Nevertheless, this is an important book in both its conclusions and in its creative use of recently opened archival records. Stueck sets a high standard for those who will follow.

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GARY MAY. *China Scapegoat: The Diplomatic Ordeal of John Carter Vincent*. Washington: New Republic Books. 1979. Pp. 370. \$15.95.

This is an important book. It traces the career of a China specialist in the American foreign service whose activities spanned the crucial decades of the 1920s through the 1950s. John Carter Vincent, as the book demonstrates, may have played more critical roles than almost anyone else in the evolution of a policy toward China during and immediately following the Second World War. And for those roles he was destined to be persecuted and humiliated by an American public that was unprepared to accept a successful Communist revolution in China.

The book naturally divides into two halves, the first tracing Vincent's career in China and, during the 1940s, in Washington, and the second dealing with his ordeal through an interminable series of loyalty hearings. But the two halves are held together through certain perceptions of China, the United States, and Vincent himself and his family that remain remarkably consistent. In Gary May's skillful narrative, what stands out is Vincent's conviction that China, like any other country, had the

potential to develop along liberal, democratic paths. Such thinking made him critical of the Kuomintang and hopeful that a third force would ultimately emerge, subordinate to neither the Nationalists nor the Communists, that would unify the country and establish close ties with the United States. Vincent was keenly interested, during the war, in encouraging the emergence of "liberal elements" in Chinese society, for he believed that the Communists were "not democratic in our sense of the term." After 1945, he adamantly opposed assisting Chiang Kai-shek, and his persistence in this stance is credited by the author, quite correctly in my view, with having prevented a more massive military intervention by U.S. forces in China. Unfortunately, neither China nor, as it became increasingly clear, the United States, was ready for a liberal solution. Vincent underestimated the growing influence of military thinking in postwar American foreign policy and misjudged the temper of his own countrymen who were ready to equate liberalism with communism and therefore with disloyalty and subversion. He was attacked as an architect of a policy that had brought about the downfall of the Kuomintang and was ultimately forced to resign from the State Department. The sheer injustice of such an outcome is fully and convincingly documented here; the reader is aghast at the way in which congressional committees and loyalty boards ignored or twisted evidence, relied on questionable testimonies, and conducted their investigations to reap political benefits.

That an intelligent, open-minded, conscientious official like Vincent should have been so cavalierly persecuted, and that none of his superiors came to his rescue, was a tragedy for America. But he had too stable a self-perception to be driven to bitterness or despair. In the end it may have been his liberal personal philosophy that saved him psychologically and intellectually, although the same liberalism was his undoing politically.

In following this intensely fascinating story closely, on the basis of FBI, Civil Service Commission, State Department, and other files that were opened up only recently, May may have given us the most thought-provoking study in recent years of Sino-American relations.

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VICTOR S. NAVASKY. *Naming Names*. New York: Viking Press. 1980. Pp. xxiii, 482. \$15.95.

Nostalgia addicts have painted the post-Hiroshima years as a halcyon era. In reality, these were troubled times when prosperity and quiescence made

men forget such festering sores as urban blight and racism. The Cold War hysteria intensified such internal tensions. Nowhere was this situation more apparent than when the House Committee on Un-American Activities (HUAC) attempted to purge alleged Communist influence from the motion picture industry. In 1947, HUAC hearings resulted in jail sentences and blacklisting for a group of screenwriters, directors, and producers all because they refused to tell whether they were or were not Communists. Four years later, the committee resumed their inquisition, and witnesses could invoke the Fifth Amendment and receive probable blacklisting or confess and furnish names of suspected Communist comrades.

Victor S. Navasky, editor of *The Nation*, describes these "degradation ceremonies of humiliation" in what he labels a "moral detective story." Of all the works written on this subject, his is the only one to focus on and analyze the whole question of informing, the character of the informers, and the nature of their testimony. Disdain of informers is an established tenet of our value system. The morality of such people is only one of the philosophical considerations Navasky deals with, perhaps with too much interesting speculation. The predicament of such people in a democratic milieu is presented objectively and with a certain dramatic flair. Navasky demonstrates how conscience, loyalty, and absolutism interrelate and conflict. The facts are there, but there is also provocative conjecture as well on the entire question of betrayal and forgiveness. The volume is therefore an amalgam of history, philosophy, and sociology.

An important chapter probing the motives of informers rests upon in-depth interviews with them. Although the oral testimony is fascinating, the analysis is equally important. Much of it is burdensome, and rationalizations obviously abound, but Navasky's journalistic skill and sensitivity clarify the story. The author is both dispassionate and compassionate. This is admirable because his sympathies lie with the resister. Yet Navasky posits the intriguing question of whether both informer and resister have not paid their full measure.

The present work is definitely a contribution to the understanding of the Cold War psyche and the adjustment of the movie industry to it. It is refreshing to read a work that is not of the unabashedly nostalgic, uncritical variety of movie trivia. On the basis of the implications in Navasky's book, one is entitled to wonder whether the purpose of the Hollywood investigations involved more than seeking out subversion but included as well concern over the general impact of the movie industry. Because of the subject, this volume cannot be considered a pleasant one, but for scholar and serious movie fan alike, interested in better comprehending the mo-

tion picture industry during the McCarthy era, *Naming Names* remains a singular source.

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TRUDY HUSKAMP PETERSON. *Agricultural Exports, Farm Income, and the Eisenhower Administration*. Lincoln: University of Nebraska Press. 1979. Pp. xii, 222. \$15.95.

Trudy Huskamp Peterson's *Agricultural Exports, Farm Income, and the Eisenhower Administration* is a clear, careful, and competent study of agricultural trade, a complex and important preoccupation of foreign and domestic policy in the Eisenhower years.

Taking office after twenty years of Democratic rule, the Eisenhower administration inherited many federal programs and policies that it abhorred. To the incoming Republicans, agricultural policy—a complicated, confusing, and contradictory welter of programs that shared a commitment to little but government intervention—exemplified the worst legislative and administrative aspects of the New Deal. Secretary of Agriculture Ezra Taft Benson and other policy makers found price-support programs particularly distasteful, but they recognized the impossibility of destroying the basic elements of a structure supported by powerful and vigilant interest groups. Instead, the USDA favored the development of expanded foreign markets that would absorb the existing surplus, provide outlets for future production, and thus end the necessity for domestic price supports. In order to increase exports, the Eisenhower administration used a complex variety of devices, including loans, barter agreements, donations, export subsidies, and sales for foreign currencies.

The attempt to devise an international solution to a national problem exposed contradictions between the foreign and domestic policies of the Eisenhower administration. The USDA saw agricultural exports primarily as the solution to internal economic and political problems, but the State Department was disturbed by the implications of the policy for the American position of leadership in the non-Communist world. Diplomats worried that, while a vigorous export policy might benefit the United States temporarily, it could offend developed allies, weaken underdeveloped ones, and invite retaliation from both. In addition, the State Department argued that indiscriminate exports to Communist countries or to neutrals could prevent the use of food as an American weapon in the Cold War. By 1960 this controversial export policy had failed. Allies and potential allies had been offended, but large new markets remained elusive, the surplus

grew larger, and the government was more deeply involved in agriculture than ever before.

Peterson tells this story carefully and well, and her conclusions are judicious. She concentrates almost entirely on export policy, failing to cover the subject of farm income promised in her title, and her prose tends to descend to the stiff and graceless standard set by her sources. Despite these minor criticisms, however, Peterson has served us well by exploring a very difficult, complicated, and important subject in a clear and intelligent manner.

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MANSEL G. BLACKFORD. *Pioneering a Modern Small Business: Wakefield Seafoods and the Alaskan Frontier*. (Industrial Development and the Social Fabric, number 6.) Greenwich, Conn.: JAI Press. 1979. Pp. xx, 210. \$26.50.

This is a fine case study of the organization, development, and final merger of a company dealing in Alaskan king crabs from the 1940s to the 1970s. Mansel G. Blackford draws some parallels with American frontier activities such as mining. A better parallel, however, might have been one of the studies in the fishing industry—lobstering.

Wakefield Seafoods seems typical of many small businesses in an exploitive field. The type of organizational structure is similar to that of larger firms. In 1945, a group of men in their thirties with navy war experience or shipping experience incorporated Deep Sea Trawlers. The group was led by Lowell Wakefield and investors having previous knowledge of Alaska, the fisheries industry, or the Wakefield family. Among the backers we see familiar business names, like Weyerhaeusers, from the Seattle, Tacoma, and Chicago areas. One of the original supporters and an early boat captain was William Blackford, the author's father. Mansel Blackford has combined family information, oral history work, and written sources into a careful study of this company.

Here we see the classic story of a resource, king crabs, just waiting to be caught. The hopes of quick profits were soon dashed. Equipment had to be developed, as well as processing facilities, to take advantage of the new markets for frozen products after the war. This is the personal story of men in isolated portions of Alaska, their equipment, and the successful organization that developed. The years from 1945 to 1952 were years of struggle, 1953 to 1963 were ones of expansion, and 1964 to 1968 were years of restructuring after the earthquake and the merger with Norton Simon.

In these years there unfolds the story of the dwindling of a resource, the competition among firms,

labor problems, and international competition with the Japanese and the Russians in the 1950s. For some, the most interesting aspects of this book will be the political activities. Like the lobster industry in the East, the king crab industry in Alaska had to cope with changing regulations at the state, federal, and international level.

The physical presentation of the volume is excellent. There are two good sketch maps of Alaska. Each chapter has subdivisions and a conclusion. There are extensive footnotes showing a variety of reference materials. Some problems exist in the abbreviated interview references and the limited index. These are rather minor errors that do not really mar this excellent small business study. More such studies are needed to show the development of American business and to reflect on the dangers of small businesses where luck seems to play as much a role as good planning or solid financing.

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NELSON MANFRED BLAKE. *Land into Water—Water into Land: A History of Water Management in Florida*. (Florida State University Book.) Tallahassee: University Presses of Florida. 1980. Pp. viii, 344. \$19.95.

Throughout most of its history, Florida has been in the peculiar position of having too much water rather than not enough. Not until the 1960s did events conspire to convince the citizenry that "water was not a nuisance to be gotten rid of but a resource to be husbanded" (p. 166). Nelson Manfred Blake, who addressed the question of urban water supply in a book published a quarter of a century ago, has returned to his earlier interest in water resource management and has traced the tortuous evolution of Florida's policies from developmental boosterism to an ecology-based environmentalism.

The story, covering the years 1821–1977, is dominated by two themes: efforts to build a canal across Florida to reduce the distance between the Gulf and the Atlantic as well as to lessen the likelihood of shipwrecks along Florida's perilous coast and attempts to drain the state's vast swamps in order to create new farmlands. Boosters never realized the first goal, although they had agitated for a canal as early as 1821. The dream of a canal came close to fruition in the 1960s when some construction was actually undertaken, but environmentalists and hostile politicians successfully halted the project in the 1970s. Drainage schemes have been somewhat more successful, although the results have not always been as beneficial as the boosters had anticipated. Nevertheless, so much swampland was endangered that environmentalists lobbied for preservation of as much of this native Florida habitat as



possible. The major result of these efforts was the creation of the Everglades National Park.

The bulk of the book deals with the exploitation of both land and water resources, but Blake also discusses the wide-ranging environmentalist legislation passed by Florida during the 1970s. Yet the author clearly believes that these gains are not enough and warns that the subterranean streams (aquifers) that supply most of Florida's water needs are in danger and must be protected against depletion and pollution.

Blake strives to present all sides of the issues of land and water use in Florida, but his environmentalist sympathies are evident. He alludes to the "prison-like walls around Lake Okeechobee" (p. 3), refers to one canal project as a "plumber's dream" (p. 261), and even entitles one chapter "Environmentalists to the Rescue." Nevertheless, the treatment of these explosive issues is scholarly, well documented, and largely without rancor. This well-written volume is a good example of how history can be brought to bear on contemporary problems in an informative and judicious manner; it represents an important contribution to the literature of environmental history.

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MICHAEL FOLEY. *The New Senate: Liberal Influence on a Conservative Institution, 1959-1972*. New Haven: Yale University Press. 1980. Pp. x, 342. \$17.95.

Michael Foley has re-examined the conventional wisdom surrounding the operation of the United States Senate. The aim of his book was to study carefully the main premises of William S. White, *Citadel*, and Donald R. Matthews, *U.S. Senators and Their World*, in order to see if these two analyses still hold true. The catalyst for the study was the numerical growth of liberal senators after 1958, which the author believes transformed the body into a "new Senate."

The method of inquiry is direct and to the point. Foley describes the "old Senate" and its structure relating his observations to the work of White and Matthews. A discussion of the political environment between 1959 and 1972 follows. The next four chapters provide the heart of the work. The 1958 congressional elections brought a doubling in the number of liberal Northern Democrats that began the unraveling of the conservative Senate. The number of liberals for the period covered in the study was thirty-four, which comprised a substantial bloc willing to challenge the established folkways of the Senate. One of the most important of these customs to be attacked was "apprentice-

ship." Traditionally freshman senators assumed insignificant roles. Foley maintains that this changed noticeably after 1959. He cites the elevation of freshman Harrison Williams as chairman of the subcommittee on migratory labor in 1959 and the appointment of another new senator, Eugene McCarthy, as chairman of the select committee on unemployment as examples of this transformation. Other practices also came under scrutiny and change. The liberal Senate of the 1960s was a body quite different from earlier decades, yet all had not changed. The liberals, according to Foley, had become more traditional and had begun to demonstrate more conservative views about the folkways of the Senate. Throughout the 1960s, however, the liberals dominated and were a potent legislative force and impulse for change.

Senate liberals did alter the body in the 1960s and the views of White and Matthews did need to be modified. "Structure," "culture," and "style and functions" were affected, thereby creating a new organization. The case is persuasive because the evidence is impressive. Numerous interviews, use of the Guttman scale to deal with voting, and a clear, well-written presentation make this a very valuable contribution. The only question that one might pose hypothetically is, What will become of the "new Senate" in the 1980s?

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SEYMOUR MAXWELL FINGER. *Your Man at the UN: People, Politics, and Bureaucracy in Making Foreign Policy*. New York: New York University Press; distributed by Columbia University Press, New York. 1980. Pp. xx, 320. \$26.50.

Seymour Maxwell Finger examines the place of the UN in American foreign policy, the way the U.S. mission to the UN functions, and traces the history of the American mission from its inception through Ambassador Andrew Young's period of service. To a highly personal evaluation of each U.S. permanent representative are added views of the American delegation and staff in each case. Conclusions as to options the United States has in the UN and other international organizations conclude the author's considerations.

Finger has given an "insider's" dimension to his description of the United States-United Nations mission gained from fifteen years of working with the UN and personal knowledge of many of the U.S. permanent representatives. This personal perspective has not diminished his treatment of key issues affecting the United States at the UN. It may, however, give too sympathetic a tone to the book in

general. Andrew Young's controversial career is dealt with fairly, and his contributions to peaceful resolution of African problems is properly praised. In fact, the most pregnant statement in the book grows out of Young's and McHenry's approach to the Third World: "Many serious conflicts of interest between the Third World and the West remain and the debates will often be sharp. Yet the record of recent years indicates that there is virtue in patient understanding combined with hard-headed bargaining rather than belligerent confrontation" (p. 286).

The greatest contribution this work makes is the profile summary of "desirable traits" of the U.S. permanent representative to the UN, a cabinet-level appointment (pp. 306-08). Someone should copy these pages and send them to every U.S. president-elect.

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ARCHIMEDES L. A. PATTI. *Why Viet Nam? Prelude to America's Albatross*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xx, 612. \$19.50.

Shortly before his death, I had a chance to "de-brief" Bertram Wolfe on his travels with Ho Chi Minh in the late 1920s. They spent approximately three months together, for the Comintern, touring the Soviet Union. Wolfe recalled that "Ho was the sharpest, most callous communist organizer I ever met. While in public he spoke with fierce pride as a Vietnamese nationalist, in private he readily acknowledged that this was all for show. What mattered most to him was power—gaining it and holding it—and he pledged to manipulate any cause and anyone to these ends."

Until the publication of *Why Vietnam?*, this was a side of Ho that received scant attention. Archimedes L. A. Patti has now given us the first real glimpse of Ho as a communist agent, and it coincides with much that I learned from Bert Wolfe.

The other novel feature of Patti's book is what it reveals about the state of American intelligence on Indochina on the eve of our fateful involvement. This is principally contained in a chapter on "The Unintelligible Intelligence Community," which demonstrates that U.S. intelligence on the Far East was a "jumble" and destined (by prejudice and organizational shortcomings) to remain so.

*Why Vietnam?* is a first-rate and highly readable memoir of the U.S. involvement in the struggle for Indochina from 1940 to 1954 in which the author was personally involved (as head of the OSS-Indochina mission). As such it is a welcome complement to the "standard" historical works of Jo-

seph Buttinger, Robert Randle, Bernard Fall, and Wesley Fishel.

The principal shortcoming of the book lies in the lessons the author draws from the experience. These are contained in a brief April 1980 postscript that overdraws parallels to current problems and issues (Iran, Afghanistan, and the Carter doctrine). The real value of the book is its vivid demonstration of quite another lesson: look before you leap.

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## CANADA

BARRY M. GOUGH. *Distant Dominion: Britain and the Northwest Coast of North America, 1579-1809*. (University of British Columbia Press, Pacific Maritime Studies, number 2.) Vancouver: University of British Columbia Press. 1980. Pp. 190. \$19.95.

*Distant Dominion* is the second of a series, "Pacific Maritime Studies," published by the University of British Columbia Press. It also constitutes an antecedent to Barry M. Gough's previous work, *The Royal Navy and the Northwest Coast of North America, 1810-1914*.

The northwestern coast of the North American continent was the last portion of the earth's temperate zones to be brought within the orbit of the emerging European-based empires. Until the development of steam-driven ocean vessels in the latter half of the nineteenth century, geography, winds, and ocean currents made the northwest coast one of the more remote and inaccessible shorelines, reached most easily from Europe by a long, tedious voyage down the Atlantic, around the Cape of Good Hope, and across the Indian and the vast stretches of the Pacific oceans. Indeed, the first commercial connections were by European merchants in the fur trade based in Bombay and Canton. That the British were able to establish a firm and lasting hold on a large part of the northwest coast was due in no small measure to their commercial enterprise by sea and the strength of the Royal Navy. A related factor was the emergence, after the middle of the eighteenth century, of Britain as a strong international power. Also important, in a negative sense, was the inability of any of Britain's rivals for the area—Russia, Spain, and the United States—to marshal simultaneously the requisite naval strength and commercial resources.

The voyages of Drake in the sixteenth century and Cook and Vancouver in the eighteenth century

are given prominent roles by Gough in his account. Drake's discoveries in 1579 established the English claim against other European nations, but it was a claim the English did not follow up until Cook was dispatched to find a northwest passage into the Pacific for merchant vessels. With the publication of his *Voyages* in 1784, due notice was given to the timber resources and the furs of the northwestern coast. The first important consequence was the rise of the maritime fur trade with Asian markets. This trade was to bring the remote coastline into world commerce and to intensify international rivalry for the region. By the last years of the eighteenth century, British traders operating from Indian and Chinese ports had outstripped their Spanish, Russian, and Yankee rivals. They benefited from a grand project put in motion by the British government, a program for the extension of British settlement and trade into the eastern seas and along the rim of the Pacific. The intervention of the government in the controversy with the Spanish over Nootka Sound in 1790 was decisive. Russia did not have sufficient naval power in the region, the Yankee traders had not yet arrived in sufficient numbers, and France did not support Spain. The convention forced upon the Spanish by Britain to end the dispute over Nootka Sound laid the foundation for a Canada extending from ocean to ocean. British desire for commerce and British naval superiority along with a firm commitment by the government in London had proved decisive in establishing British dominion. It remained for Captain George Vancouver to put to rest the myth of a northwest passage in the southern latitudes and for Canadian fur traders moving overland from the Athabasca country to consolidate Britain's hold. British interests in the maritime fur trade and the whaling industry could now operate in the northwest Pacific without fear of molestation. By the second decade of the nineteenth century, this remote coastline had been brought within European influence. What lay in the future was the exploitation of the great mineral, agricultural, and forest resources of the Pacific cordillera in the industrial age.

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HUGH N. WALLACE. *The Navy, the Company, and Richard King: British Exploration in the Canadian Arctic, 1829-1860*. Montreal: McGill-Queen's University Press. 1980. Pp. xxix, 232. \$21.95.

The search for the Northwest Passage, the Franklin expedition, and the mysteries surrounding its disappearance continue to fascinate historians and arctic buffs. In this work, Hugh N. Wallace provides

the reader not so much a rehash of events surrounding the disaster but a critique of British arctic exploration in the mid-nineteenth century. The book might disappoint those not well acquainted with the topic, simply because the author has decided to concentrate on the state of the art and leaves out much of the color and dash of the explorations themselves. By doing so, though, Wallace has produced a crisp, well-researched perspective on the techniques, strategy, and logistics of arctic travel and discovery in an era of rapidly changing technology. The book should be of benefit to all students of arctic or naval history.

Despite the title, the focus is really on the Royal Navy and its hydrographic office. True, if Richard King had been heeded, a northwest passage might have been discovered by the early 1840s, and he did predict later the whereabouts of Franklin's crew. Unfortunately, no one listened to a man whose arctic career ended in the 1830s and who moved outside the naval and commercial establishments. One may question whether he should be considered a central character, since his role, even the author demonstrates, was reduced to letter writer and critic of British arctic policy. King may have been right, but he remains an interesting and neutral factor in the scheme of things.

The role of the "Company" (the Hudson's Bay Company), its effort, and contributions are partially obscured by Wallace's emphasis on the navy's well-publicized arctic program. Like King, the company believed in the light, indigenous method of arctic travel that used inland river and coastal routes. The company and its servants could not employ any other approach for the obvious reason that it had vested commercial interests in practical means of travel and survival in the higher northern latitudes. It too was interested in exploration and lifting the curtain on its arctic hinterland, but its purpose and perspective were different and hence would have little bearing on the admiralty approach.

The navy's task in a search for the Northwest Passage—aside from producing a make-work project after the peace of 1815—was to unravel the mysteries of Canada's complex arctic coastline and archipelago. To do the job properly, two modes of exploration were necessary. First, initial discovery usually involved the navy's traditional deep-sea, heavily equipped ship, which was geared to more southern climes where the navy could operate in open sea. When, however, its ships were blocked by ice—as they had been on a number of occasions—the Royal Navy appeared reticent or incapable of adapting to the circumstances. Second, initial discovery should have been followed by light, fast-moving parties that could use inland or coastal routes to achieve their arctic objective. Instead, the Royal Navy tragically clung to exploration and sur-

veys involving vast support structures, the latest European technology, and naval officers well trained in the traditional scientific disciplines. Little consideration was given to polar ocean conditions, and much attention was paid to providing or duplicating aboard ship the environment of the homeland. The navy's difficulty stemmed partly, as Wallace so aptly points out, from a deflection of its original purpose (warfare) and partly because it "sailed" in a new element (ice) as opposed to sailing on water. Wallace is correct in pointing out that a different approach would have been necessary to do the job quicker and to avert tragedy—much along the lines suggested by King and practiced to a polished state by the Hudson's Bay Company. This was not to be, nor could it have been. It was, simply put, not the way to do things in the queen's navy.

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GEOFFREY BILSON. *A Darkened House: Cholera in Nineteenth-Century Canada*. (Social History of Canada, number 31.) Buffalo, N.Y.: University of Toronto Press. 1980. Pp. viii, 222. Cloth \$15.00, paper \$5.95.

Geoffrey Bilson's imaginatively titled book appears at the end of a quarter-century of cholera studies that have portrayed the disease in Russia, Europe, England, the United States, Latin America, and in some areas of Asia. Most of these studies have focused on sociopolitical or cultural issues and have contributed relatively little to epidemiology. On the contrary, medical understanding of cholera has been essential to interpreting its social significance. Bilson's book fits part of this pattern. It provides a narrative overview of each cholera outbreak in Canada, recording the public hysteria it generated, the quarantines that were established, the social confrontations it promoted (in this case, French-Canadian resistance to English immigration was a particularly troublesome and significant aspect), the mortality and its consequences, and the short- as well as long-term influence on sanitary policies and public health. In addition, there is detailed information on the ideas and reactions of the physicians involved; Bilson also offers some interesting comments on the emergence of professionalism and on cholera's role in strengthening the idea that medicine's professional legitimacy depended on science. Only a few violent incidents marked the Canadian cholera experience, a fact that Bilson ascribes to the absence of a strongly centralized bureaucratic-military system of sanitary controls. This conclusion deserves a more thorough exploration than it receives.

Bilson's research combines rich archival material with contemporary newspapers and journals. He also fully controls the current secondary literature.

Canadian historians will undoubtedly welcome a book that covers an important event in Canadian history in such detail. Historians hoping to gain further insight into the problems of society and disease, however, will be somewhat disappointed. The author gives relatively little detailed information on the class and ethnic composition of the communities affected, on the distribution of domiciles and the density of population, on occupational groups, living patterns, and personal values, and on the correlation of these and other phenomena with cholera incidence and mortality. Apart from the political issues associated with immigration and quarantine, there is virtually no attempt to analyze the interaction of disease and society. What is said on these matters is interpolated from other studies to which Canadian data are fitted.

In the end, Bilson's book provides a thoroughly documented survey of the cholera in Canada that contributes as well to the sum of information on medical ideas and the evolution of the medical profession. A deeper understanding of cholera's meaning for the communities it visited and any analysis of the symbiosis between the characteristics of those communities and the cholera's incidence or impact must await another and different study that focuses its attention on the localities attacked.

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MICHEL HORN. *The League for Social Reconstruction: Intellectual Origins of the Democratic Left in Canada, 1930-1942*. Buffalo, N.Y.: University of Toronto Press, 1980. Pp. xii, 270. \$20.00.

The League for Social Reconstruction was dedicated to fostering socialism in Canada through research and adult education. Despite its claims to be nonpartisan, it was soon associated in the public mind with the CCF, the fledgling Canadian socialist party. The public was right. The leading figures of the league, as Michiel Horn shows, shifted easily from working on LSR publications to drafting manifestos, party platforms, or pamphlets for the CCF. Indeed, the league itself petered away after a decade of modest activity largely because the pressures for direct political action left its most active members with less time for and less interest in the league.

The league, however, is a window on the ideas of prominent democratic socialists in Canada during the depression. These ideas, as Horn makes clear, were neither original nor coherent. There was plenty of moral fervor in the denunciation of monopoly capitalism and in the vision of a cooperative and egalitarian society, but the analysis of Canadian society and the prescriptions for changes were



eclectic, more often Fabian than Marxist, with traces of guild socialism and reform liberalism as well. The major contribution of the league, as Horn suggests, may have been no more than that it drew academics into active politics on the side of social change.

The league has some limitations as a focus for studying socialist ideas in Canada. The key members of the league lived in Toronto and Montreal, and even these men retained their own distinctive views. The opinions of Underhill, Scott, Forsey, Spry, Gordon, and others do not add up to anything that could be identified as a league perspective. Horn is very good on the views of each of these individuals in turn, but the sum of their ideas is not a coherent version of socialism.

It is also difficult to put the league in the context of the left in Canada. No study of a group of central Canadian academics can be an adequate survey of the "intellectual origins of the democratic left" in Canada, because it cannot weigh the contributions of westerners such as Salem Bland, William Irvine, or Ernest Winch, and it must necessarily ignore any leftist ideas in the Maritimes or Quebec. But if the league was not the democratic left in Canada, it was certainly the most effective instrument of socialist analysis and propaganda in the 1930s. By focusing on the structure, the activities, and the ideas of the leading members of the league, Horn has produced a monograph that is a major contribution to our understanding of socialism in Canada.

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## LATIN AMERICA

RAMÓN EDUARDO RUÍZ. *The Great Rebellion: Mexico, 1905-1924.* (Revolutions in the Modern World.) New York: W. W. Norton. 1980. Pp. xii, 530. \$24.95.

Ramón Eduardo Ruíz inherited his father's skill as a spellbinder. Just as the author fell under the charm of his father's recollections and anecdotes of his native Mexico, he captivates the reader with his tale of the Mexican Revolution from 1905 to 1924. He deftly guides readers away from traditional interpretations with his graceful writing and arguments, persuasive by their casual nature. After all, who would interrupt a master storyteller to quibble that "once upon a time" fell on Thursday, not Friday?

This comment should not suggest that Ruíz has concocted a Mexican bedtime story. He demon-

strates hard-nosed scholarship. Based on research in Mexican and U.S. archives and his firm command of historical literature, he presents a provocative synthesis of part of the Mexican Revolution and calls it a rebellion. He writes with such facile strokes that he is off to another point before the reader questions this or that. Take his periodization. He concludes his study in 1924 because he argues that the northern rebels had consolidated their power with the victory over the De la Huerta-led challengers. This dismisses the Cristero rebellion and minimizes the revolution's creation of party machinery to transfer peacefully the presidential chair in 1928. Others would argue that the revolution survived to 1940, and others have the notion it endured even longer.

Or take Ruíz's bewitching conclusion that no revolution occurred in Mexico, only a great rebellion. After a perceptive discussion of revolution and its consequences in the modern world, he does allow that Mexico probably endured the last of the French-inspired revolutions that brought a new class to power and strengthened the capitalistic economy. This happened in Mexico, as he demonstrates—the Mexican national bourgeoisie came to power and, despite rhetoric festooned with talk of dirt-farmer alternatives, dug its heels even deeper into the capitalistic soil.

Still the Mexican experience, despite lacking Marxist slogans, has much in common with the great struggles of this century. Nationalists fought foreign influence, experts, and investments; thus the rhetoric became stridently anti-Yankee. The social dimension aimed, as Tannenbaum noted, at small changes to improve village life; that these eventually failed does not wreck their revolutionary character. And the revolution despite itself promoted modernization that destroyed the traditional society just as effectively and finally as the disregard and exploitation of the old regime.

Ruíz has carefully crafted chapters on revolution, the major revolutionaries and their goals, and the "Juans" who took up arms to follow them. He examines labor and villagers. He combines expert scholarship with his father's ability to tell a captivating story. This is an excellent book; when Ruíz typed the title, he must have looked up and winked.

WILLIAM H. BEEZLEY  
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JANAÍNA AMADO. *Conflito social no Brasil: A revolta dos "Mucker," Rio Grande do Sul, 1868-1898.* São Paulo: Edições Símbolo. 1978. Pp. 304.

Brazil is famous for its messianic and millenarian movements, and most have received detailed scholarly study; until now, however, no one has closely

examined the Mucker movement, a rebellion of German-Brazilian Pentecostals in Rio Grande do Sul (1874). In addressing this problem, Janaina Amado provides the best-documented case study of millenarianism in Brazilian history.

The author begins with an examination of the German peasant culture in Rio Grande from the 1820s and traces the impact of a new market economy on German-Brazilians in São Leopoldo county, where the rebellion occurred. Local society became more stratified between 1840 and 1870 as commerce developed between São Leopoldo and the provincial capital, Porto Alegre. Transport innovations were the driving force: new steamship runs on the Rio dos Sinos, and above all the extension of the railroad to São Leopoldo in 1874, the year of the uprising. Such changes rapidly drove up land values and helped forge a new bourgeoisie in the community, while simultaneously dispossessing the peasantry.

The Muckers were led by Jorge Maurer, an illiterate carpenter turned faith healer in 1868, and his literate wife Jacobina, who dominated the sect in its crisis years (1872–74). The Muckers (that is, “hypocrites” to their enemies) first fell afoul of local merchants and clergy, then of the local law. The group re-created an egalitarian community in expectation of the millennium. Ultimately, misperception of the sect at provincial and national levels, exacerbated by atrocities by and against Muckers in São Leopoldo, led to a military expedition to repress the cult. As at Canudos, Brazil’s most celebrated millenarian community, an initial military expedition against the Muckers was defeated. This raised new alarms in Porto Alegre, and a larger expedition resulted in the destruction of the group—an outcome reminiscent of Canudos, where some of the same officers served two decades later.

Many Muckers, however, escaped death, and from police records before and after the rebellion (if such it was), Amado has gathered more social data on the group (including family ties) than exist for other classic millenarian sects. For 169 rebels (of 250 who took up arms), arrest records show that the overwhelming majority were illiterates, and most were Brazilian born but German speaking. The majority had lost their farms in the years just before the rebellion.

Although the research is informed by sociological theory and a comparative literature, a few things might have been explored more thoroughly. Amado does not pursue the possible role of the Religious Question, though she discusses the rise of Masonry after 1870. Furthermore, she does not relate the rebellion to two other developments in 1872–74—the introduction of the metric system and the taking of Brazil’s first census. Both events brought “external” authority directly into village life, as the contempo-

aneous Quebra-Quilos revolt in the Northeast shows. Yet these lacunae detract but little from a fine monograph.

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JOSEPH L. LOVE. *São Paulo in the Brazilian Federation, 1889–1937*. Stanford: Stanford University Press. 1980. Pp. xx, 398. \$25.00.

Invariably, rapid economic development is regionally uneven. The fact of regional disparities has given rise to theories of their origins and practical programs for their elimination. Usually, interests in the stagnant “periphery” try to martial the national government to redistribute the benefits of development in their favor. Conversely, interests in the advanced “center” attempt either to capture these same forces in order to exploit the periphery or to weaken the national government to forestall redistribution. The outcome is unpredictable a priori.

There has been relatively little empirical research on the political economy of regional disparities in developing countries. This gap makes a trilogy on Brazil’s states quite welcome. Joseph L. Love’s study of São Paulo is the last of three independent but coordinated efforts to be published. (The two others are John D. Wirth, *Minas Gerais in the Brazilian Federation, 1887–1937* [1977] and Robert M. Levine, *Pernambuco in the Brazilian Federation, 1887–1937* [1978]). For political analysis the state in Brazil is a logical unit, because the expressions of power, such as ownership of public lands, fiscal authority, and the control of higher education (important in the socialization of elites), are organized on that basis. The period 1889–1937 is easily bounded, because its beginning marks the end of slavery and the centralized monarchy; its end marks the collapse of the coffee economy and with it state autonomy and the ideology of agriculture as Brazil’s natural vocation.

A better description of São Paulo in this formative period of Brazil’s modernization would be hard to find. Love crams between two covers a superb survey of the state’s geography, demography, economy, religion, art, and social structure. Although others have written on these topics in English and Portuguese, Love provides a masterful synthesis as well as much new material. Particularly interesting is a chapter on culture in its broadest sense, from the assimilation of Italian immigrants and acquisition of taste on the part of the nouveau riche to the status of women and modernism. Another interesting chapter and an appendix deal with the state’s political elite, its career patterns and genealogy. A chapter on state-federal relations provides some insight on benefits of political decentralization to the

center's elite. The elite was able to retain the gains from coffee export taxes, yet nationalize the losses inherent in the coffee subsidy schemes. A chapter on fiscal federalism collates valuable data on patterns of state spending. São Paulo's elite used their enormous fiscal base to support elementary education, public health, and a police force at relatively high levels.

Love's book will undoubtedly prove more useful to specialists on Brazil than to the general scholarly audience. Although the author whets the reader's appetite about the broader issues of regionalism in the excellent introduction, he draws few striking conclusions. Nor is the wealth of data on elites fully exploited to provide new general insights.

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GEORGE REID ANDREWS. *The Afro-Argentines of Buenos Aires, 1800-1900*. Madison: University of Wisconsin Press. 1980. Pp. xiv, 286. \$21.50.

This is a fascinating, highly readable work with important implications for the study of race relations far beyond the narrow confines in time and space of Buenos Aires, 1800-1900. There have already been quite a number of studies of shifting quality on the history of the "Afro-Argentines" who formed a major element of the population of the city and province of Buenos Aires in 1810 but mysteriously disappeared from view in the course of the century. This phenomenon has variously been ascribed to the demise of the slave trade, to a combination of low birth and high death rates of the group (the latter mainly a result of heavy male casualties in the wars, 1810-60), and to miscegenation. George Reid Andrews, however, is the first scholar to undertake a systematic, comprehensive study of the subject. His careful, sophisticated quantitative analysis of available sources conclusively proves that in absolute terms Afro-Argentine demographic decline did not set in until the second part of the century. Furthermore, using a wide range of sources (such as photos) Andrews shows the persistence of an active and self-

conscious (though internally split) community of colored until, at the least, the early years of the twentieth century. His chapters on the "black legions," the various community organizations comprising religious lay brotherhoods (*cofradías*), "nations" (associations based on real or fictitious tribal basis), and mutual aid societies, as well as "black" newspapers, 1850-1900, are simply brilliant.

From a strictly scholarly point of view, however, Andrews's adherence to the peculiar United States conceptual "black" and "white" dichotomy is to be regretted. It tends to obscure his main point of explanation of the final demographic decline, that is, the process of "whitening" (pp. 89-90). Andrews is aware of the pitfalls and occasionally comments upon them. I believe that his argument would have been much clearer though, had he consistently used the same Negro (*moreno*) and mulatto (*pardo*) categories that appear in the sources themselves until at least the mid-nineteenth century. The ambivalent term *trigueño* ("wheat colored"), whenever applied to persons of African descent, undoubtedly referred to light mulattoes only.

Andrews keenly and convincingly analyzes the clash taking place between Afro-Argentines and immigrants and the withdrawal of the former into bottom-rank governmental jobs (they were "citizens" after all). In the context of immigration and dynamic socioeconomic change from 1880 onward, the middle-strata Afro-Argentine efforts to become integrated with "white" society failed. Instead, a racial dichotomy (not unlike the North American one) gradually emerged. Already reduced to numerical as well as socioeconomic insignificance, Afro-Argentines of whatever shade of skin would be referred to and think of themselves as *gente de color*. The role of "racism" (as defined in the United States of today, not Argentina) in producing this new pattern is quite clear. Thus Andrews, when comparing his findings on race relations in Buenos Aires with those of Brazil and the United States (as analyzed by Carl Degler) perceives "a bleakly depressing historical cul-de-sac for Afro-Americans of all languages and cultures," as he aptly puts it (p. 206).

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## Collected Essays

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These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ȘTEFAN PASCU *et al.*, editors. *Nouvelles études d'histoire, publiées à l'occasion du XV<sup>e</sup> congrès international des sciences historiques, Bucharest, 1980*. In two volumes. (Académie des Sciences Sociales et Politiques.) Bucharest: Editura Academiei Republicii Socialiste România. 1980. Pp. 326; 385. 26 L.; 30 L.

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- ROTH, RUSSELL. *Muddy Glory: America's "Indian Wars" in the Philippines, 1899-1935*. West Hanover, Mass.: Christopher. 1981. Pp. 281. \$12.95.
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- STOKES, JOHN W. *The Man Who Lived Two Lives*. 2d ed. New York: Vantage Press. 1980. Pp. ix, 94. \$6.75.
- STROUT, CUSHING. *The Veracious Imagination: Essays on American History, Literature, and Biography*. Middletown, Conn.: Wesleyan University Press. 1981. Pp. xiv, 301. \$17.50.
- THOMAS, SHERRY. *We Didn't Have Much, But We Sure Had Plenty: Stories of Rural Women*. Garden City, N.Y.: Anchor Press/Doubleday. 1981. Pp. xviii, 185. \$7.95.
- THOMPSON, DENNIS L. *Taxation of American Railroads: A Policy Analysis*. (Contributions in Economics and Economic History, number 34.) Westport, Conn.: Greenwood Press. 1981. Pp. xvi, 186. \$29.95.
- WAGENKNECHT, EDWARD. *Henry David Thoreau: What Manner of Man?* (New England Writers Series.) Amherst: University of Massachusetts Press. 1981. Pp. 211. Cloth \$12.50, paper \$5.95.
- WATSON, RICHARD A. and MICHAEL R. FITZGERALD. *Promise and Performance of American Democracy: With an In-depth Analysis of the 1980 Presidential Contest*. 4th ed. New York: John Wiley and Sons. 1981. Pp. x, 727, 55, 25. \$19.95.
- WEBER, DAVID J. *The Taos Trappers: The Fur Trade in the Far Southwest, 1540-1846*. Reprint. Norman: University of Oklahoma Press. 1980. Pp. xiii, 263. \$6.95.
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## CANADA

*Canadian Historic Sites: Occasional Papers in Archaeology and History*. Volume 23, *Blockhouses in Canada, 1749-1841: A Comparative Report and Catalogue*, by RICHARD J. YOUNG; *Gaspé, 1760-1867*, by DAVID LEE; volume 24, *Second Empire Style in Canadian Architecture*, by CHRISTINA CAMERON and JANET WRIGHT; volume 25, *Gothic Revival in Canadian Architecture*, by MATHILDE BROUSSEAU. Ottawa: National Historic Parks and Sites Branch. 1980. Pp. 190; 247; 208. \$12.00 each.

## LATIN AMERICA

PHILIPSON, LORRIN and RAFAEL LLERENA. *Freedom Flights: Cuban Refugees Talk about Life under Castro and How They Fled His Regime*. New York: Random House. 1980. Pp. xxix, 201. \$12.95.

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."*

### TO THE EDITOR:

President David Pinkney made so many statements in his presidential address, "American Historians on the European Past" (*AHR*, 86 [1981]: 1-20), which are subject to doubt, that I feel compelled to question them before they are sanctioned by silence.

In the first place, American scholars displayed an interest in European history at a much earlier date than Professor Pinkney thinks. Smith College had a course in the 1890s on Europe's past, given by Charles Hazen, and so, too, did Yale. By early in the twentieth century, we did have Shailer Mathews's *The French Revolution* (1900), W. M. Sloane's *Life of Napoleon Bonaparte* (1910), and James Harvey Robinson's *History of Western Europe* (1902). Before World War I, Charles A. Beard and Robinson were propounding the doctrine of the "New History," which had a striking similarity to the principles found in the historical philosophy of Lucien Febvre. Not a word about any of these men from Pinkney.

On the "honor roll of historians," Professor Pinkney had many curious omissions. By way of example, let me cite the fact that no mention was made of Robinson and Beard, of Carlton Hayes, past president of the American Historical Association, nor of James T. Shotwell, author of several essays on history and editor of the famous *Economic and Social History of the War*. In his list of historians who came from Europe during World War II, he omitted the name of Solo Baron, the author of the great history of the Jews, but put in the late Franz Neumann, who was a political scientist and by no stretch of the imagination a historian. He paid no attention to great teachers like Carl Becker, disregarded studies of the philosophy of history, and did not even refer to studies similar to his. His piece needs revision.

SHEPARD B. CLOUGH  
Columbia University

### PROFESSOR PINKNEY REPLIES:

I am pleased and flattered that my old and much-admired friend, Shepard Clough, read my Presidential Address with such critical interest. I did not, of course, pretend to offer—in fifty minutes—a comprehensive history of the writing and teaching of modern European history in the United States, and I necessarily omitted mention of many important historians. The *Review's* readers will, I am sure, share my gratitude to Professor Clough for his taking time to tell us of those he thinks especially deserving of mention.

DAVID H. PINKNEY  
University of Washington

### TO THE EDITOR:

Samuel Kinser, in his article "Annaliste Paradigm: The Geohistorical Structuralism of Fernand Braudel" (*AHR*, 86 [1981]: 63-105), has rightly questioned the value of Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II* (2d edn., 1966) as a model; but Professor Kinser has confined his discussion to questions of formal method. I do not challenge his choice of means to do legitimate and useful work. I merely wish to add that, if he had attended also to content, he would have had to notice the absence from Braudel's pages of the Inquisition. Whatever would Philip II have done without it?

It would seem that Braudel's notions of structure included acceptance of a leaning tower of history.

FRANCIS JENNINGS  
Newberry Library

### TO THE EDITOR:

There is apparently a belief among the scholars of East Asian history in this country that it is difficult, if not virtually impossible, for a Chinese, Japanese, or Korean to write dispassionately and objectively on the history of modern Sino-Japanese or Japanese-Korean relations. I am, therefore, especially gratified that Professor Akira Iriye, an eminent authority in the field, finds my book, *The Last Phase of*



*the East Asian World Order* (1980), "free of emotionalism, chauvinistic biases, or dogmas." At the same time, I am somewhat surprised that he nevertheless thinks that my use of certain terms reveals where my sympathies lie. In his generous review of my book (*AHR*, 86 [1981]: 192-93), Professor Iriye writes that certain nouns and adjectives, such as "talented . . . perceptive mind" and "patriotic men of intelligence and integrity," seem to be reserved for Koreans, whereas "treacherous and untrustworthy," "depravity," "predatory," and "intransigence," describe other (non-Korean) nationals. Regrettably, this statement is factually erroneous and its implication misleading.

In combing through my book, I find Professor Iriye's list of laudatory terms describing Korean figures that appear in the volume is virtually complete; I can think of only one other place (pp. 223-24) where a Korean official is portrayed in commendatory terms. I am, if anything, more laudatory in describing Japanese personalities. Examples: "remarkable thinker" (p. 80); "brilliant student of the West" (p. 84); "outstanding student of the West" and "enlightened official" (p. 94); "man of superior intelligence" (p. 106); "patriotic men dedicated" (p. 234); and "Refreshingly free of prejudice . . . took the most enlightened approach" (pp. 237-38). In every single instance in which the terms "treacherous and untrustworthy" and "depravity" or their cognates are used, they are either direct or indirect quotations from a source representing the views of an individual or group under discussion. Examples: "Korean officials believed . . . depravity of Manchu officials" (p. 29); "Japan's historical 'depredations' against China . . . the memorialists stated" (p. 72); "report added . . . Koreans were so crafty" (p. 129); and "Brandt said . . . Japanese diplomacy . . . bordered on treachery" (p. 280). I have scrupulously avoided the use of these terms in expressing or presenting *my own views*.

I have used the term "intransigence," along with such other terms as "chauvinism," "ignorance," and "xenophobia." In each instance I believe their use is correct, objective, and justified. I should further point out that I have used them perhaps more often in reference to Korea and Koreans rather than to any other country and its nationals. To cite just a few examples: "chauvinism and ignorance [in Korea] . . . produced . . . exclusionist doctrine" (p. 37); "anti-Western xenophobia in Korea" (p. 55); "Korea's past intransigence" (p. 162); and "Korea's new intransigence" (p. 222).

Professor Iriye notes that in my book "most Korean figures, as well as some Chinese and Japanese officials, are depicted as oriented toward their respective national interests." While not totally inaccurate, this seemingly innocuous remark carries an unfortunate implication. On what factual or sta-

tistical evidence, one may wonder, does Professor Iriye base such imprecise but clearly comparative and contrasting quantitative terms as "most" Koreans and "some" Chinese and Japanese?

Finally, according to my count, the term "predatory" appears ten times in my book—an incidence perhaps not high in a volume whose main text covers some 350 pages. In the first two instances (pp. 85 and 128), it is used to present, on the basis of sources, a Japanese perception of the nineteenth-century world as one dominated by predatory powers. In the remaining instances (pp. 155, 202, 205, 255, 335, and 336), the term describes Japanese or Western *diplomatic tactics or practices* seeking territorial and commercial gains or unfair diplomatic advantages from smaller or weaker nations through the use of force or intimidation. It is not used to characterize any country or people as a whole or even the foreign policy of any country as a whole. Since this is an area of subjective interpretation where historians can honestly differ from one another, the term in some cases may appear harsh or even "incongruous." Even so, the use of the term should not be treated as a "quick dismissal" or condemnation of Japan and the West, as Professor Iriye suggests. I cannot see how certain aspects of Japanese and Western diplomacy in East Asia in the late nineteenth century can escape being characterized as predatory or in similar terms, even though their practitioners honestly pursued what they subjectively considered "their respective national interests." Commenting on an innocent affair like the 1871 Sino-Japanese treaty, British historian Grace Fox writes that the terms of the treaty "indicate how much both nations [China and Japan] had learned by 1871 of Western predatory diplomacy" (*Britain and Japan, 1853-1883* [1969], 276).

I am not writing this letter to prove that my sympathies lie somewhere other than suggested by Professor Iriye. Nor do I claim that I am completely free of biases or prejudices. I write it because in this volume I have honestly endeavored to be scrupulously fair and objective.

KEY-HIUK KIM  
University of California,  
Davis

#### PROFESSOR IRIYE REPLIES:

The point I tried to make in my review of Professor Kim's excellent book is that its excellence would have been further enhanced, and its scholarly contribution greater, if the author had gone beyond presenting Western and Japanese imperialism as "predatory," to which Korean officials responded in terms of perceived national interests. These generalizations are obviously tenable, but it seems to me that the data presented in the book are too rich and

complex to be analyzed in these conventional frameworks.

AKIRA IRIYE  
University of Chicago

TO THE EDITOR:

Given Robert Soucy's published views on the French left and right, I did not expect that he would feel unrestrained enthusiasm for my book, *The Action Française and Revolutionary Syndicalism* (AHR, 85 [1980]: 1207-08). I was dismayed, however, not that he disagreed with me, but that he misrepresented what I actually said. I did not intend, as Soucy would have it, to attack syndicalism as a "potentially corrupt mass movement." In fact, I specifically stated that my purpose was *not* to look at syndicalism as a mass movement but to study the political and intellectual interaction between royalist and syndicalist elites.

"We learn a good deal," Soucy complains, "about a handful of syndicalist renegades but little about the syndicalist response [to the royalists] as a whole. . . ." This, it seems to me, misses the point. We have already been told ad nauseam, in the standard accounts, that the "syndicalist response as a whole" was negative. Moreover, I clearly stated that the royalist overtures were never taken up by the majority of the syndicalist leaders, much less by the rank-and-file. Should it really surprise Soucy that a book about syndicalist-royalist relations would focus more on those syndicalists who had relations with the royalists than those who did not?

Soucy's coup de grâce comes in his penultimate paragraph where he lists all of the "evidence" that "contradicts" my "image of syndicalism." Where was this evidence found? All of it comes from my own book, a fact which Soucy admits without embarrassment. I stand castigated not because I did not present all of the pertinent evidence on both sides, but because I did not "dwell" on one of the sides.

For the record, let me summarize my position as it concerns the syndicalists: (1) a small group of syndicalist leaders had either overt or covert relations with the royalists; (2) these relations had some impact not because of the numbers involved but because of the factionalized and beleaguered state of the CGT; (3) to the degree that the syndicalist leadership as a whole was criticized, it was not because they were flirting with the royalists but because of a certain lack of scruple evident in their attack on parliamentary socialists (most notably, the CGT's support of Delaisi's vicious campaign against Jaurès's *L'Humanité*, laced as it was with antisemitic overtones).

In addition, Soucy takes the liberty of correcting me on certain points without checking his facts first. For example, I am chided for neglecting to mention

that when the Action Française criticized the parliamentary right's "stodgy conservatism" it meant only "its political not its economic conservatism." In fact, the neoroyalists did criticize the parliamentary right's economic conservatism, particularly their insensitivity to the plight of the working class and, after 1908, their hostility to striking workers. In another place, Soucy claims that I am misleading the reader by quoting "one hyperbolic soul" who argued that the violence of the CGT and the Camelots du roi was in fashion in 1911. "It certainly wasn't in 'fashion,'" Soucy tells us, "with the overwhelming majority of the French electorate." I never said it was, nor did my "hyperbolic soul" (Marc Sangnier); in the next sentence, Sangnier states specifically that he is not referring to the mass but to the intellectual elite. And, in point of fact, I quote not one "soul" but several.

Finally, Soucy takes me to task for my treatment of certain early fascists at the Action Française. He is not happy that I find them "serious" and "sincere." Here, I am neither surprised nor offended by his disclaimer. Certainly the question of fascism is controversial, and I can hardly put myself above criticism. Yet I do attempt to argue why opportunism is not very convincing in the case of someone like Valois through a careful biographical reconstruction. I would be more than willing to hear why Soucy finds my reconstruction unconvincing. But to simply dismiss my argument abruptly and somewhat magisterially and to say, without further comment, that "rank opportunism" seems a more likely explanation does not strike me as altogether fair minded.

PAUL MAZGAJ  
University of North Carolina,  
Chapel Hill

#### PROFESSOR SOUCY REPLIES:

Paul Mazgaj's letter states that he did not intend to attack syndicalism as a potentially corrupt mass movement. Mazgaj's book (p. 218) states that "the seamier underside of heroic syndicalism has received little attention" and that "as this study makes plain, socialist politicians were not always the perpetrators and syndicalists not always the victims; clearly there were cases where the lines of aggression were reversed." On page 219 he argues that elements of the pre-1914 revolutionary left anticipated later attacks on reform socialists as "social fascists" in "a pale, nonetheless real, foretaste of the scurrility and ultimate destructiveness of a revolutionary purism that viewed ethical decency as a form of decadence." On page 222 Mazgaj leaps from the individual to the general (without previous justification) when he asserts that the cases of Valois, Berth, and Janvion become "an almost

archetypal pattern" illustrating "the facility with which the transmigration might be made from the extreme Left to the extreme Right." That such a transmigration was, in fact, the exception not the rule (and that the exceptions abandoned much of their leftism in the process) renders its "facility" rather meaningless. This is the kind of verbal slipperiness I was referring to in my review.

Mazgaj's letter says his purpose was not to look at syndicalism as a mass movement but to study the interaction between elites. But his book implies much more, mostly through a heavy reliance on what I call "iffy" history. A typical example of this is on pages 214–15. "Had the *Guerre sociale* not reversed itself, Janvion, Pataud, and their allies would have had the most popular newspaper of the revolutionary Left as a platform." But the *Guerre sociale* did reverse itself. "The forces of resurgent nationalism and an embittered revolutionary left would have been combined and sealed with the bonding agent of popular resentment against the Jews." But they did not combine and seal. "This possibility . . . was certainly a real one." But this real possibility did not occur. I assume, incidentally, that "the forces of an embittered revolutionary Left" would have included some of the rank-and-file; otherwise why the term "forces"?

I fail to see why I should be embarrassed by the fact that some of Mazgaj's major conclusions are contradicted by his own evidence.

To say that relations between certain syndicalists and royalists had "some impact" begs the question as to the *extent* of that impact.

I would be happy to be provided with "facts" showing that neoroyalist criticism of the parliamentary right's economic conservatism was more than just empty rhetoric. Verbal sympathy is cheap; raising workers' wages, shortening their working hours, and increasing taxes on the wealthier in behalf of the poorer comes harder. If the neoroyalists supported the latter measures, such facts would indeed be interesting to know.

To say that violence was in fashion with only the "intellectual elite" is still hyperbolic, of course. The looseness with which Mazgaj uses the term is paralleled in the book by his comment that French fascist leaders of the interwar period attempted to "revivify" (p. 222) left collectivisms with popular nationalism—whatever revivify means. If it means that these fascists were committed to socioeconomic collectivism (as opposed, say, to cultural collectivism in the service of petty bourgeois economic conservatism), then Mazgaj will have difficulty with mountains of evidence to the contrary. And, at the risk of being magisterial, I find George Valois no exception.

ROBERT J. SOUCY  
Oberlin College

#### TO THE EDITOR:

Ruth Pike could have enriched us all with a thoughtful, critical review of my book, *Crime and Society in Early Modern Seville* (AHR, 86 [1981]: 406–07). I had hoped that she would share her scholarship in Spanish history. Instead, she offers us the following.

First, she accuses me of "faulty translations" and errors that "may be found on almost every page of the book." To support this allegation, she cites two cases described on pages 85 and 281, respectively, that were based on Cristóbal de Chaves' *Relación de las cosas de la cárcel de Sevilla y su trato*. A major problem is that Professor Pike refers to this document by another name, and we do not even know if this is the same document from the Archivo Municipal de Sevilla or a variant copy.

Professor Pike quotes from the Spanish and interprets the case I described on page 85 in such a way as to show that I mistranslated the document. Professor Pike cites Chaves, part *two*, however, for her reading of the case; my footnote clearly shows that I referred to an entirely different case that is found in Chaves, part *one*. The fact that the Chaves document has no page numbers complicates the issue, but Professor Pike must surely realize that Chaves referred to more than one case involving prostitution and the royal prison.

The case I described on page 281 is about a young woman who was placed in a brothel by Juan de Molina. Professor Pike presents us with a fragmented quotation in Spanish, but again she does not identify which copy of the document she is quoting, nor does she give us a translation. Moreover, the quotation she cites in no way contradicts the story of a young servant girl. If she had read the entire passage, she would certainly have understood why I interpreted the case as I did.

Second, Professor Pike asserts, "Perry states that the records of the charitable dowries granted by Juan de Mañara [sic] from 1660–70 [sic] are particularly valuable for a study of the underworld, but she does not submit them to a systematic analysis." Once again, Professor Pike has mistakenly described the documents I used. The donor's name in this case was *Miguel* de Mañara, and the dowries were granted in the years 1666–70. Even more grievous, the reviewer seems to have stopped with the bibliographic essay in my book before she actually read the book to see how the Mañara papers were used. I used the dowries as an example of a particular form of charity, and I did not submit them to a systematic analysis because they are numerically insignificant. Much more important in the Mañara papers was a survey of poor people in 1667. I used material from this survey to discuss poverty and living conditions, particularly for women and children.

Third, Professor Pike accuses me of ignoring the "extant data" available on crime in early modern Seville, but she gives us no indication of what this data might be. As a Fulbright scholar, I spent an academic year carefully researching the archives of Seville and Andalucia. The only archive I can think of that is not cited in my work is an archive of protocols. I did not use material from this archive because it refers mainly to civil process and did not appear to me to be helpful in describing a relationship between crime and the society of Seville.

Fourth, Professor Pike declares that "extant judicial records" show that most criminals in Seville were not professionals or marginal people. Once again, she fails to identify her documents. In addition, her statement is confusing. On the one hand, it supports a major point in my book, that crime was *not* neatly segregated into a professional class in this city. On the other hand, she does not explain what she means by "marginal people." I argue in my book that people from all walks of life easily drifted in and out of a criminal subculture. Is Professor Pike attacking this or supporting it?

Fifth, Professor Pike criticizes my book for "oversimplifications" and "a simplistic concept of the underworld." I would welcome the suggestions of a senior historian in how I might deepen my understanding of crime and Spanish society in the early

modern period. Professor Pike, however, offers no suggestions.

Finally, Professor Pike complains of "errors in the spelling of Spanish names and carelessness in accentuation." From her own work, she must know that spellings and accentuation vary widely in documents of the early modern period. In every case, I used the spelling and accentuation of the specific document cited.

Professor Pike would have us believe there is nothing of academic value in my book, but many people disagree with her. The four other reviews of my book have been very laudatory. There are weaknesses in my book, but it deserves serious and scholarly discussion.

MARY ELIZABETH PERRY  
*University of California,  
Los Angeles*

#### EDITORS' NOTE:

The two sequences of articles devoted to comparative history in theory and practice (*AHR*, 85 [1980]: 753-857, 1055-1166) have generated an unusually large number of letters to the editor. The editors have decided, for coherence, to publish all of the letters and replies together in the December 1981 issue of the *Review*.



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# American Historical Association

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Founded in 1884. Chartered by Congress in 1889  
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**CORRESPONDENCE:** Inquiries should be addressed to the Executive Director at 400 A Street, S.E., Washington, D.C. 20003.

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# American Historical Review

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Single copies of the current issue and back issues in and subsequent to volume 81 (1976) can be ordered from the Membership Secretary of the Association at \$10.00 per copy. Issues prior to volume 81 should be ordered from Kraus Reprint Corporation, Route 100, Millwood, N.Y., 10546.

Correspondence regarding contributions and books for review should be sent to the Editor, *American Historical Review*, 914 Atwater, Indiana University, Bloomington, Indiana 47405. Unsolicited book reviews are not accepted; a statement concerning reviewing policy will be found in the issue for December 1970 (75: 1889-91). A statement concerning the kinds of articles the *AHR* ordinarily will and will not publish appears in the issue for October 1970 (75: 1577-80). The entire text, including quotations and footnotes, of article manuscripts must be prepared in double-spaced typescript, with generous margins to allow for copyediting, and submitted in duplicate. Footnotes should be numbered consecutively throughout and should appear in a separate section at the end of the text. Other guidelines for the preparation of manuscripts for submission to and publication in the *AHR* will be sent upon request. Articles will be edited to conform to *AHR* style in matters of punctuation, capitalization, and the like; and the editors may suggest other changes in the interest of clarity and economy of expression. But such changes are not made without consultation with authors. The editors are the final arbiters of length, grammar, usage, and the laws of libel.

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**AMERICA: CHANGING TIMES,**  
2nd Ed.

Volume I: To 1877

Volume II: Since 1865

**Combined Volume****Charles M. Dollar**, General  
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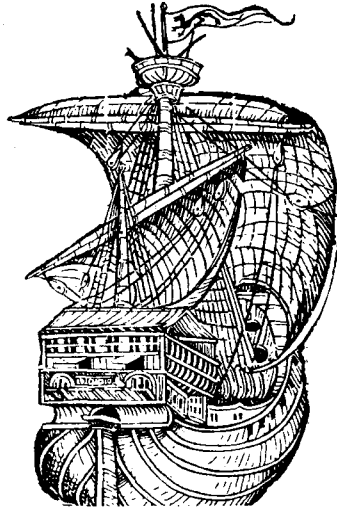
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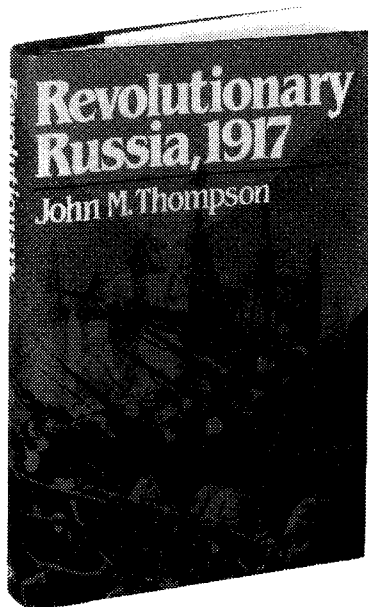
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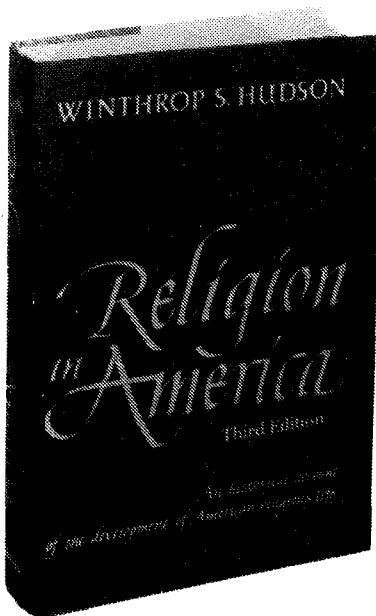
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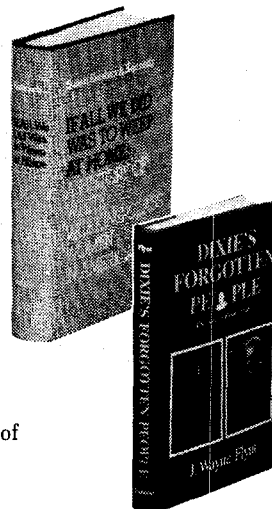
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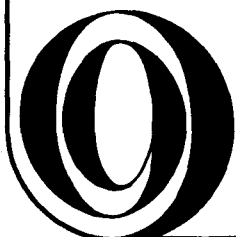
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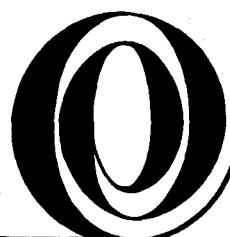
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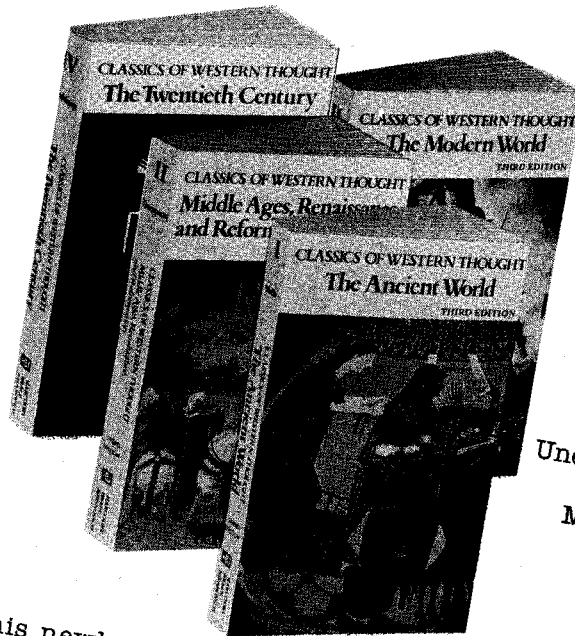
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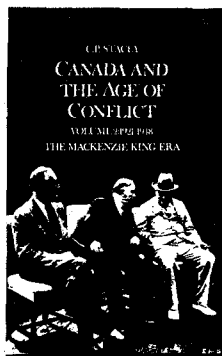
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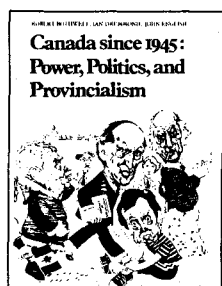
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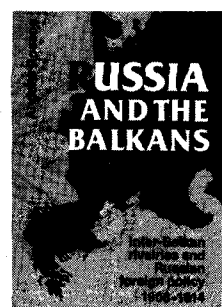


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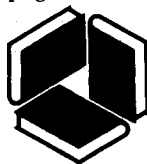
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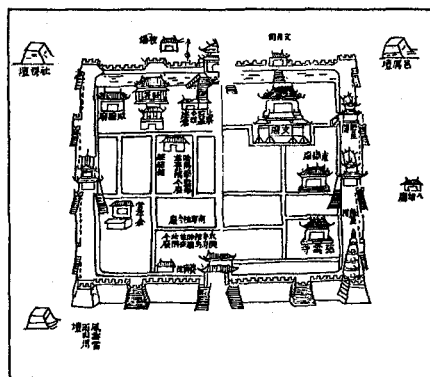


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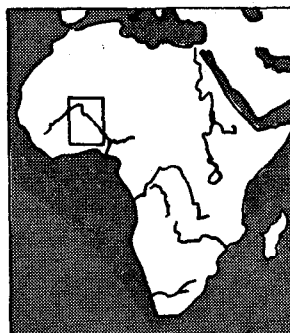
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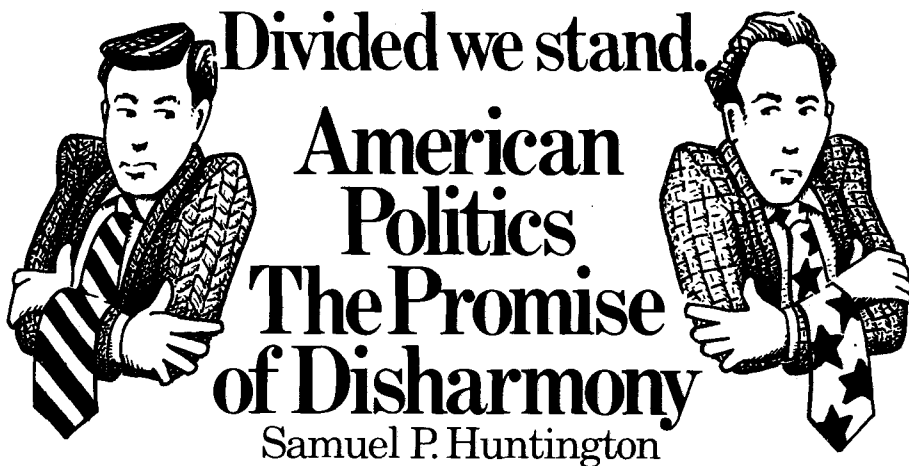
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